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ABSTRACT

This essay addressed the question of to what extent does school segregation and school desegregation policy shape residential segregation. Census data, ethnic segregation, voluntary self-segregation, and attitude surveys are discussed in the residential segregation section. The Wolf and Lebeaux study is critically appraised in reference to school and residential segregation. The consequences of school desegregation policies for school and residential segregation is reviewed. The research of James Coleman, Sara D. Kelly, and John A. Moore is analyzed. Contrary to Coleman's assessment of white flight to the suburbs, the author indicates that upon critical evaluation of the research data pertaining to the 21 school districts massive desegregation has had little or no effect on white flight to the suburbs. Finally, the effects of school desegregation and school and residential segregation is assessed. Tables of black and white family income for 1969, Boston's Irish and Italian surnames by selected voting districts, and black, Irish, and Italian population data are presented. The appendix includes the Morgan vs. Kerrigan case and a paper presented by James Coleman to the American Educational Research Association. An extended list of references is also included. (JP)

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School Segregation, Residential Segregation: Some Speculation

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INTRODUCTION

I have had little knowledge of school segregation[1] other than the idea that it was shaped by residential segregation. Consequently, I was intrigued by this opportunity to write a personal, speculative essay which reversed my usual view and required me to ask, to what extent do school segregation and school desegregation policies shape residential segregation? Moreover, this was to be speculation based on evidence from the literature, and in my own research.

It became obvious that tne usual answers to this question were given in terms of the current school busing dispute, and varied from "everything" to "nothing". Those who believe "everything", range from those who believe whites would leave the school system for private or other public suburban ones, to some who claim it would attract whites; I read no one who wrote that Blacks might reject school integration. Those who believe "nothing" tend to believe whites would either grudgingly accept or even welcome school integration, and it would have no residential effect. The debate usually focuses on city and suburb, race and social class, with some reference to private versus public schools. Thus, residential segregation is subsumed under the question of "white flight" from city to suburb in the literature, and is so treated in this essay. Actually, population movement (residential shifts) is not the same as segregation; for example, whites may move from a white neighborhood in the city to a similar one in a suburb. Nor is a shift in enrollment from public to private school necessarily related either to population movement or to residential segregation. Often these are all confounded.

I begin from the assumption that we can look at the two extremes of school desegregation, the "voluntary" and the "involuntary". The "voluntary" in which individual parents decide to participate in whatever plans are available, by definition have no effect on residential population movement or segregation. At the other extreme, I would define as "involuntary" those plans, such as Boston's, where individual parents have no choice except to participate in the plan, or else move to another school district or enroll their child in private schools (or both). I will discuss only the "involuntary".

As I have already said, population movement and residential segregation are not identical phenomena, and I shall try to consider them separately. In the current debate, migration is cast as people moving from central city to suburb, - i.e. "white flight" - which is how I will consider it.

Segregation, as we shall see, is a more elusive phenomenon to operationally define, and it sometimes seems that every investigator

creates a new definition. For the most part I will mean it as reflected in the measure most commonly used by sociologists, the "Index of Dissimilarity". We shall discuss this later, but it will suffice for the present to say it is (at best) an ordinal scale from 0 (no segregation) to 100 (complete segregation) which gives the percentage of a city's white (or Black, or Irish, or any other group) which would have to move in order for their residences to be distributed (usually across census tracts) in the same manner as some other group. When I depart from this usage, I will usually be talking in an impressionistic quasi-cartographic sense of, say, Blacks in the central city and whites in the suburbs.

Having said this, I can over-simplify to report a general consensus of the scholarship which uses various definitions of school and residential segregation and desegregation. With the exception of the largest 22 school districts, over which Professor James Coleman and his associates differ from a number of critics, my interpretation of the literature is that in general, involuntary school desegregation leads to no additional "white flight" or changes in residential segregation patterns. Coleman's projection for these 22 districts is that "large scale" involuntary school desegregation may very likely lead to "white flight"; in my view, for current public policy issues, there are actually only a dozen of these districts where the issue is still in doubt (for example, we need no longer anticipate for Boston). Also, among the remaining districts smaller than the largest 22, I do not think any of the protagonists would claim it is utterly impossible for "white flight" to occur upon school desegregation, but only that the odds are against it. The specific odds would probably vary according to the investigator, but since none of the protagonists explicitly consider this question for these smaller districts, I will not discuss any further what is only my own conjecture. Among the largest 22 districts, I have concluded that Coleman's projections have merit. This is not because I believe Coleman is inherently "right" and his opponents "wrong", but rather because to my mind, the various protagonists use different assumptions and definitions with which to talk past one another; I have seen nothing of evidence or logic in which they invalidate one another. For example, in the literature there is little consensus on definitions of "large scale", "involuntary", "school desegregation", or "white flight".

If I conclude Coleman has no superior metaphysical validity, why then do I propose his merit? Simply on consideration of the pragmatics of future public policy. That is, Coleman's assumptions and definitions are neither better nor worse than his opponents, and they differ only with respect to projections for 22 districts, so there is no intellectual reason to ignore him. Moreover, since his projections have all too unhappily born fruit in Boston, it would be folly to ignore him when considering the possibilities of "white flight" in other, similar school districts.

We shall discuss the debate over the 22 largest districts in some detail. But to tie school to residential segregation, we need to focus on selected aspects of each, a matter in which my own subjec-

tive judgement plays a large role. I have tried to avoid confusing latitude with license, I hope with some success.

There are two aspects of this general debate which tacitly shape my own discussion; first, the social, legal, and educational issues involved in the quest for social justice in race relations which is the basis for the most interesting part of the debate; and secondly, the violent polemic which characterizes the debate itself. For the first, I have had to exclude it entirely for lack of both my own knowledge and the space it would take to discuss it. For the second, it has led me to include far more detail - perhaps verbosity- and longer quotes than I should wish, in the hope that where I differ from the reader or err in understanding, it may still shed more light than heat on a question which has seen too much of the latter. In order to relieve this necessary but boring detail, several sections end with summaries, which in conjunction with the Conclusion section should enable readers to skip those parts which they consider talmudic exegesis or jesuitical casuistry.

Also, I felt I could not altogether ignore issues of residential segregation itself, for I believed many writers err because of their assumptions. They begin with the idea that whites are undifferentiated except for social class so that white ethnics, such as Irish and Italians, maintain no voluntary segregation from one another. Another key assumption seemed to be that Blacks had no tendency toward voluntary self-segregation. Obviously much racial segregation is involuntary and reflects white prejudice. But how much? One asks different kinds of questions and gets different kinds of answers if one assumes racial segregation is caused on one hand only by bigots, snobs and mortage lenders, or on the other, that voluntary self-segregation plays a major role, at least in some cities.

Consequently, I have begun with a prefatory section on residential segregation before turning to issues of residential and school segregation. This in turn leads to a section of questions of "white flight", most concretely centered upon the research of James Coleman and his associates. Finally, I will present my thought about these issues as they concern Boston, and then turn full circle to some speculation about the issue in general. In all of this I shall try to review the basic issues by using personal judgement to select key examples of research or criticism, without making an attempt to comprehensively review all the literature on a given subject.

1. RESIDENTIAL SEGREGATION

Before I begin discussing the two issues which concern ethnic and Black voluntary segregation, and attitude surveys as evidence, a few comments on definition and data may be in order. In relation to the meaning of residential segregation, we exclude segregation with little or no territorial basis (for example, that whites and blacks have different kinds of jobs).

Within the residential, there are questions of what kind of evidence do we have, and how do we make use of it. Restricting ourselves to non-cartographic quantitative studies, we find a nearly universal reliance upon statistics provided by the U.S. Decennial Census of Population and Housing, our one source which provides statistics for small areas such as census tracts. The census itself has its limitations, and the U.S. Bureau of the Census is a leader in specifying these; for example, for the total U.S. Negro population, males in their 20's have been under-counted by about 20 percent (Siegel and Zelnick, 1966) and the 1970 Census is only slightly better. (U.S. Census 1973).

More important, the census can ask only a limited number of questions. Among these, for example, are ones of education, occupation, and income. This has given rise to a veritable mass production industry of computerized studies of education, occupation, and income. Many of these studies are quite useful. For example, in residential change of neighborhoods from white to Black (or "population succession"), we can be reasonably sure that the first Blacks to move into a hitherto white area tend to be better educated than the extant white population (Duncan and Duncan, 1957; Taeuber and Taeuber, 1965) and this is a different phenomena than population succession which involves Puerto Ricans (Kantrowitz, 1973; Rosenberg and Lake, 1976).

But the census does not ask questions of religion (nor in my opinion, should it), so that census data cannot help us consider the possibility that in such cities as New York, this initial movement of Blacks into white areas tends to be into Jewish rather than Catholic areas (a speculation I have heard many times, but about which I am doubtful). In this regard, I suspect these initial moves are made by Blacks who are not only better educated than average (which we know from Census Statistics), but also "ethnically" different--perhaps Roman Catholic or segments of only certain Protestant populations. For the most part in this paper, I shall limit ethnicity to European foreign born and descendents, but I will also occasionally allude to my basic definition which subsumes background religion, place of birth and/or territory; thus Roman Catholic Blacks or Appalachian whites are ethnics. The resident white populations may themselves happen to live along certain transit lines, or they may tend to be slavic Jews, Irish Catholics, or some other group whose better educated moved out before the first Blacks appeared. It would be absurd to expect the U.S. Census to provide data for the investigation of all such speculations, a more fitting subject for the universities.

Even if we restrict ourselves to questions for which abstract index numbers provide sufficient evidence, and for which basic statistics are available from the U.S. Census, qualitative judgment plays a large part, and may even mislead us in understanding what are the important questions to ask in the study of residential segregation.

In my view, the importance of judgement is highlighted in a recent publication (Hawley and Rock, 1973) of the National Academy of Sciences. The U.S. Department of Housing and Urban Development asked the National Academy of Sciences to address the basic question (p.2) "What is known about the feasibility and desirability of "social mixing" in residential areas?" This volume is a compendium of 5 original papers which admirably carries out its charge. But it turned out that the original basic question devolved to an evaluation of the scholarship on just racial and social class factors in involuntary segregation from the view of aiding government policies to promote integration. It entirely excluded voluntary white ethnic or Black separatism. The aim is laudible and the execution competent. But for the present, I would direct my attention to how the basic question devolved after careful examination by very able researchers: two distinguished scholars from a Social Science panel of same; the Division of Behavioral Sciences of The National Research Council; the National Academy of Sciences; the National Academy of Engineering's Advisory Committee to the U.S. Department of Housing and Urban Development; and "key officials" of the U.S. Department of Housing and Urban Development.

The Social Science panel itself turned the basic question into three (p.6-7): "What is meant by "social mixing"? Is it feasible and desirable? What knowledge exists in the behavioral and social sciences that bears upon these questions?" These in turn lead to a series of bibliographic essays on race and social class in segregation.

I think this is admirable, for the American Black has been the object of abominable prejudice and suffers discrimination in residence far beyond the simple fact of poverty; common sense and scholarly documentation alike illustrate that economics explains little, as for example, we do not find rich and poor blacks and whites integrated. But I would pose the question, what about other factors which were omitted from consideration in these essays? After all, the basic question excluded none. I would like to consider just two exclusions which I consider important to the basic question: is there self-segregation among those of similar religion and national origin -- i.e., Irish from Italian Catholic - which is independent of racial considerations; second, to what extent would Blacks maintain a voluntary self-segregation from non-Blacks in the absence of discrimination? There is nothing inherently wrong in excluding these questions from consideration. But what I find interesting are the reasons for the exclusion.

When it comes to the first, the existence of voluntary self-segregation among ethnic whites, the editors (p.8) were explicit -- inter-ethnic segregation is a phenomenon of the past, irrelevent to contemporary life:

> Similarity, not differences, characterizes residential neighborhoods. People of like income, occupation, and stage of family life cycle tend to cluster together. As has been widely noted, families traditionally select homes in areas that will bring their children in contact with others of similar, or perhaps slightly higher, economic status. The

heterogeneity of the society at large may even foster increased homogeneity in residential neighborhoods.

Historically, ethnicity and race have also played a role in shaping the pattern of residential distribution of the population. In varying degree, immigrant groups retained a sense of communal solidarity derived from a common language and culture. These groups tended to cluster together until their members were ready to disperse amid the larger society.

Historical analogies between the blacks in America and these other ethnic groups are inappropriate in several respects. Since color is visible, blacks are more effectively barred from open access to housing. Separate, identifiable black communities have persisted over a longer time and on a larger scale than those of any other ethnic group. Today, socio-economic differences among blacks are nearly as varied as those among whites. The absolute number of poor whites is much greater, but the proportion of poor among blacks exceeds that among whites by a large margin. At the same time, the number of middle-class blacks is large and growing. Their income is rising and their occupations are becoming more diverse. Family residential requirements parallel those of whites in many respects, and status needs are similar.

I disagree with Professors Hawley and Rock's conclusion that self-segregation among white ethnics is not a viable force. As of the date their book was written, I had presented evidence that ethnic segregation persisted for New York (1969) and Darroch and Marston had done the same for Toronto, Canada (1971). Of course other research reaches an opposite conclusion, such as Lieberson (1963), and Taeuber (1964). However, see Taeuber (1970) for another assessment. There is nothing wrong with Hawley and Rock's judgement; I just disagree with it. Such people as the Boston Irish do exist according to what Bostonians tell me, and what we read in the media. A growing body of studies (for example, Bleda, 1975; Guest and Weed, 1976) also suggest ethnic existence. I can understand Hawley and Rock's view, for when I first saw the high inter-ethnic segregation index numbers for New York of 1960, I spent nearly a year searching for reasons why the evidence before me was not so; errors in calculation, the preponderance of immigrants, the lack of any direct evidence of the third generation, etc. It was only after months of futile attacks on my own data, did I reluctantly come to believe my previous ideas were wrong, and that I had been looking at Roman Catholic ethnicity as the white counterpart of Ralph Ellison's "Invisible Man". But for our present purpose it is not important whether Hawley and Rock are "ultimately" right or wrong. What is important is the realization that the exclusion of the white ethnic was a matter of judgement. For those who believe Hawley and Rock were wrong, a major consideration for the basic question "What is known about the feasibility and desirability of "social mixing" in residential areas?", was overlooked, hardly a reassuring prospect for those with a belief in rational planning.

My second concern, Hawley and Rock's view of to what extent would Blacks maintain a voluntary self-segregation from non-Blacks in the absence of discrimination, cannot receive so succinct a discussion, for they do not dismiss the question as specifically as they did the white ethnic. This topic is the other side of the coin from the white ethnic voluntary separatism, but since it does not enter the school desegregation question so directly, I mean to raise it only for purposes of highlighting the role of judgement, and only in this section. Because the book lacks an index, I may have missed some points, but I hope I have used representative illustrations and have not quoted out of context.

In my judgement, the authors do not believe voluntary self-segregation among Blacks is a meaningful phenomenon. As they state on p. 8:

> Some black spokesmen seek the development of community power and the improvement of housing and other services in central city areas with large concentrations of low-income groups. Yet middle-income blacks appear to seek access to housing of their own choosing wherever it may be located. Given access to a good-quality living environment, people of similar socio-economic status do not make choices reflecting either racial solidarity or racial prejudice.

On the other hand, the authors do recognize the possibility of Black voluntary self-segregation on p. 11:

> Although mixing is frequently discussed in terms of minority movement into majority residential areas, a multiplicity of other possible patterns may be envisioned. Actual housing choice is exceedingly complex. For instance, black middle-class families may indeed seek to move into white suburbs, or they may seek out other black middle-class areas and thus form black suburban enclaves.

One may continue to catch at these straws in the wind, but I think in the end, a convincing case for the authors' basic disbelief in voluntary Black self-segregation can be made from two of their own summaries. On p. 15, they summarize the 5 "factors that may determine the feasibility and stability of mixing" -- and voluntary Black self-segregation is not among them. Moreover, they end their Introduction Chapter with a list of 19 "findings" (p. 19-20) upon which the Social Science Panel of the National Academy of Sciences agreed. For example, Number 1 reads:

> For both blacks and whites the quality and convenience of housing and neighborhood services take precedence over racial prejudice in housing decisions.

While the last, number 19, is:

> Experiments in socio-economic mixing at a number of different scales could help to provide a better basis for policy formation. The same procedure would be valuable with respect to racial mixing.

Nowhere in these 19 points is there any mention of voluntary self-segregation among Blacks. All in all then, I conclude they implicitly reject it. I think this rejection flows naturally from their rejection of white ethnicity; they believe populations sort themselves out in the main, by social class, race, and stage of the family cycle. Thus, whites are homogeneous except for social class differences. But I assume that anything important in everyday life will most likely be reflected in face-to-face social life, which in turn moves people to live within some proximity. For example, liberal upper-class Jews in New York's Westchester County may believe in and practice residential integration with others, but not to the extent that it undermines the population basis for the local Synagogue; similarly liberal upper-class Quakers of the same persuasion live in some relationship to their Meeting House, rather than scattered randomly throughout the Metropolis. If another population is not "liberal" or not "upper-class", perhaps they may live more densely populated within bus rides or walks of church. Or other aspects of life such as family, kin or friends may be important, depending upon the stage in life cycle and family cycle. Given these premises, one can conclude that there is little voluntary Black self-segregation only if one believes Blacks have little in common with one another in their everyday lives.

Perhaps a hypothetical question may clarify the difference of opinion. Let us conjure an undefined "perfect" ratio scale segregation measure which varies from 0 (no segregation) to 100 (complete segregation). My view is that if one found the segregation between our liberal upper class Jews and Quakers to be 50, while that between them and Blacks to be 80, then with no prejudice or discrimination this would still place a floor of about 50 on what might be expected as the level of racial segregation, entirely on a voluntary basis. Naturally we can hypothesize any minimums we wish; moreover, empirical segregation index number (of which more later) are only conventions. But my understanding of Hawley and Rock's position concerning racial segregation is that in our hypothetical "perfect" index, the net result of voluntary segregation would result in a very low index or near zero. In imperfect reality, many cities such as New York or Boston do have average inter-ethnic indexes of aroung 50 and racial indexes of around 80. To my way of thinking, if there were no racial discrimination in such cities, the racial index would be at least 50, indicating segregation exists independently of attitudes, prejudice, and discrimination. White bias simply makes it more extreme.

But one could deny my premise, that everyday social importance is reflected in a tendency to live closer to one another. Perhaps one premise is as good as another. In fact, I believe the Hawley and Rock would deny mine, and I think they would base their objection to a large extent upon another form of evidence, the results of attitude surveys.

In fact, the lead essay in their volume is a masterful summation (from the orientation of the editors) of the results of attitude surveys over the recent decades by Professor Thomas Pettigrew, which clearly shows the tendency over the years has been for whites and blacks to give attitude survey answers more accepting of racial integration. This kind of result forms a bulwark in support of arguments against voluntary segregation, and one finds attitude survey results

consistently cited as evidence throughout the scholarly literature. Pettigrew himself treats attitudes as consequence of integration, while others treat them as causes. This is no matter for us, because my basic question is what evidence do we have that these attitude question responses are related in any way to actual residential behavior -- whether people move or not, and if so, why, when, and where.

In general, the question of the relationship between answers to attitude questions and actual social behavior has been debated for over 30 years to no empirical consensus. Naturally, the relationships vary by subject. But generally, as critics such as Professor Irwin Deutscher (1973) have maintained, in general there is little consistent evidence of such relationships. To see just how thin is the evidence of the connection when we investigate segregation, we can turn to the Pettigrew essay, for a summary of the reasons by someone who does believe in such a nexus.

We can take Pettigrew as a vital argument for several reasons: his subject is attitudes and residence, he is a foremost authority in the subject, and he deals directly with the issue (p. 39-43), although for the most part he considers a slightly different question. the relationship between "real" attitudes, and answers to survey questions. Be that as it may, he gives four reasons for belief which rest on plausibility and logic, and a fifth of empirical data. In his first four reasons, he states first, rapport between (persumably, the average paid) interviewer and respondent is good; second, attitude answers about minority groups show great consistency, even when the questions themselves differ, and when different polling agencies ask them, so answers are not likely to be just an attempt to give "respectable" answers; third, questions which do attempt to elicit "respectable" answers have not changed over the decades, so the changes in questions designed to probe the respondent's "true" attitudes must reflect real changes; and fourth, the "don't know" category in surveys serves as a catchall for responses to irrelevant or threatening questions, but the proportion of "don't know" to racial attitudes is so much lower than the proportion in answer to other kinds of questions, that racial questions are neither too threatening nor too irrelevant. These arguments are plausible but they provide no factual basis for believing attitude questions elicit "true" attitudes, much less behavior.

Pettigrew then spends several pages presenting evidence which links racial prejudice and housing integration attitude responses to actual behavior. But all of his examples of behavior are of purported voting behavior; not one pertains to residential. Why are they all voting behavior? I do not know the field, but I assume this was all he had. Pettigrew's specific examples themselves are four: first, one could predict the 1960 Presidential vote for John F. Kennedy from responses to a 1959 question "Would you be willing to vote for a qualified Catholic for President"; second, white voters in Gary, Indiana in 1968 "who supported residential segregation were more likely than other respondents to be planning to vote for Governor George Wallace for President" (several questions are presented here); third, white voters in Boston in 1965 who supported Mrs. Louise Day Hicks in interview were more opposed in their answers to housing integration (several questions are presented here); fourth, white voters in Gary, Indiana

in 1965 who tended to be anti-Mayor Richard Hatcher (the City's black mayor) also tended to be anti-integration (in response to "Would you mind if a Negro family with about the same income and education as you moved next door").

Pettigrew is one of the major scholars in the field, and we expect he has marshalled his strongest arguments, but had nothing better to present which linked attitude and residential behavior in race and segregation.

A recent article which links racial and housing attitudes with some behavior relating to residence in some way did appear in a recent American Sociological Review (Brannon, et al., 1973). The appearance in ASR itself is significant, for its editors expect ASR to publish only articles of major importance. The article itself reported an empirical relationship: among those whose answers to attitude questions (on one survey) favored "open housing", were more likely at a later canvas to involve themselves in actual behavior. What was that behavior? The signing of a petition favoring "open housing".

Thus, after 30 years of intensive use of attitude surveys, a major contribution to forging a link between attitude and behavior in housing and segregation devolves to the behavior of signing a petition! I think it fair to accept this article as a major contribution, which in turn means that the empirical evidence linking attitude surveys to anything one might call real housing or segregation behavior is pitifully thin. More detailed comments discussing the problems of linking attitudes and residential behavior can be found in the work of Professors Wolf and Leaeaux (1969:9-10). In my view, if one wishes to believe in attitudes, one is in good company. But it is both fair and objective to point out that this belief is based primarily on faith, not evidence. This is not to say that all attitude data is meaningless, but that it must be used with extreme caution, for limited purposes--as I think it has in some instances, as for example, in the work of Wolf and Lebeaux (1969) discussed next when we consider school segregation in conjunction with housing segregation.

SUMMARY

Perhaps this has been too long an introduction to housing and school desegregation, but we think it important to establish how the basic intellectual issues in the study of residential segregation itself are subjective. The basic Census statistics are limited, and field work has supplemented them little. Moreover, when we ask what are the important questions, we are skeptical of the grounds for dismissing ethnic segregation or Black voluntary self-segregation--especially insofar as those reasons rely on answers to survey attitude questions.

2. SCHOOL SEGRECATION AND RESIDENTIAL SEGREGATION

Interest in the relationship between school and residential segregation has, until recently, been notable chiefly for its absence. For example, neither in Professor Suzanne Keller's critique of what we know about The Urban Neighborhood as of 1968, nor in the latest (1974) edition of Professors Gist and Fava's Urban Society, do I find any discussions of note. If schools are discussed at all in the context of segregation or urban change, they are generally part of a long list of factors such as the business (and housing construction) cycle, fertility and migration, the family cycle, leisure values, or racial prejudice, to mention but a few. For example, Professor Herbert Gans' The Levittowners, (1967) a study of a middle-class suburban community during the late 1950's, suggests (in Tables 1, 2, and 3) that concern for schools was not a major reason for moving either from previous residence, or to suburbia. Schools were important in the community, of course, and Gans writes about them extensively, but they were one among many institutions, and far down on the list of the why, when, and where of moving.

To illustrate the relationship between school and residence, I have summarized selected parts of Wolf and Lebeaux's 1969 study of three Detroit neighborhoods during the 1960's as an example of good urban research whose findings are congruent with others.

The Wolf and Lebeaux study omitted suburbia or ethnicity. One area, Bagely, seems to be a predominantly middle class Jewish neighborhodd of 4,400 households of single family homes, into which middle class Negroes were moving. The second area we shall consider is Lafayette Park, a Central Business District urban renewal middle-class enclave of about 200 row houses and a highrise with something over 100 apartments, which was created from a destroyed Black "slum." To overcome the lack of concern with ethnicity, and to provide a convenient frame of reference, we can use one of Herbert Gans (1962:129) comments in his study of a Boston Italian "slum" during the 1950's:

> West end children can go either to the parochial or public elementary schools, but most of them must go to public ones for their high-school education. Although their parents prefer parochial to public institutions, the distinction that is of most significance to them and that creates uncertainty about schooling is between person-oriented and object-oriented education. Person-oriented education teaches children rules of behavior appropriate to the adult peer group society, and stresses discipline. This is identified with the parochial school. Object-oriented education teaches them aspirations and skills for work, play, family life, and community participation. Rightly or wrongly, this type of education is identified with the public school. It is also the source of West End ambivalence, and accounts for the placement of education in the outside world.

Gans' dichotomy makes sense as abstract extremes, except I think many middle-class education values can also be viewed as "person-oriented". For example, one reason middle-class Jewish or WASP children go to middle-class schools and colleges with large Jewish or WASP components is, as Gans defines it, to "teach them rules of behavior appropriate to (their) adult peer group society"; these schools may not stress the "discipline" of the working class Italian, but this is only an ethnic difference.

In the Bagely middle-class Jewish area, I suspect the research actually began after a substantial part of the wealthiest population had begun to move out; 34 percent of the households had children in public school and only 4 percent had children in private ones. Families with no children in school were only marginally effected by the schools, except for the issue of paying school taxes for other people's children. For them, the important thing was teenage decorum. Thus, schools had little effect on residential choice of two-thirds of this area's households, those without children.

For those with children in school, what Gans calls the "person-oriented aspects" were important. These were usually expressed in social class terms; middle-class parents value middle class schoolmates for their children. This seems to be true for both Black as well as white middle class parents; when the poor are present, they will tend avoid moving into a neighborhood, while those middle-class already resident tend to move out. This produced a small steady erosion of middle class households. However, unless the levels of dissatisfaction reach some (undefined, and perhaps undefineable) level, dissatisfactions per se seem to make no difference. For example, although there seemed to be an undercurrent of dissatisfaction or apprehension in Bagely, from Table 14, p. 39 of Homeowners between 1962-1964, the percentage of householders who moved out was similar: 15 percent with children, compared with 11 percent for those without children. Naturally, different ethnicities, races or social classes may define differently what is a reasonable level of dissatisfaction, but it does lead to a constant small erosion of middle class people with children.

Lafayette Park was a central business district middle class area, with the image and some of the reality of an inter-racial neighborhood. The role of the schools here seemed almost exactly what it was in Bagely, with the exception that in Lafayette Park there seemed to be a small minority of parents (I cannot specify their number or percentage) who specifically moved into this area because it was interracial with particular regard to education.

The high schools were another matter. There were few high school age children in Lafayette Park, and no high school (the closest was in a nearby Black slum). The middle class parents, both Black and white, shunned this alternative completely, making different arrangements.

Summary

These observations indicate to me that insofar as residential mobility is concerned, public schools are a matter of indifference to those without children. Moreover, even for those with children,

so many other factors are also involved that only when they perceive the schools as nearly totally disasterous, will they overcome the inertias of other factors to cause the precipitous moving we call "white flight" -- and even here, people who use private schools can overcome such problems.

This leads me to believe that only where there is widespread turmoil, breakdown of civility, or safety in public schools will middle-class parents precipitously move their children to private ones or move residence because of the schools. Widespread dissatisfaction without turmoil leads only to steady erosion.

Finally, few middle-class people move into an area because they are attracted to good inter-racial schools. Insofar as inter-social class schools are concerned, I expect middle-class Blacks shun them even more than do middle-class whites. Thus my understanding of Wolf and Lebeaux leads me to conjecture that only "great" disorder or "enormous" dissatisfaction will lead to noticeable "white flight", and I would speculate, to middle class "Black flight." Do school desegregation policies lead to such dissatisfaction?

3. THE CONSEQUENCES OF SCHOOL DESEGREGATION POLICIES FOR SCHOOL AND RESIDENTIAL SEGREGATION

With this understanding of residential and school segregation, we may return to the main issue: What is the effect of school desegregation upon residential segregation? In general, not much, given the discussion in the preceding section. However, the most intense interest in this question actually concerns the effects of current governmental policies to desegregate public schools, and the most common concern is that of "white flight". This commonly means white parents withdrawing their children in favor of private schools (with no residential change), or simply moving to another jurisdiction. Even more specific to scholarship is the intense debate over the findings of Professors James S. Coleman, Sara D. Kelly, and John A. Moore (which for brevity, I will usually refer to as Coleman's research): does school desegregation lead to "white flight", and if so, under what conditions?

Perhaps the most crucial arena in the contest over schools and segregation now is the Federal courts, in an adversary procedure, where strategies differ in the use of social science testimony. For example, the National Association for the Advancement of Colored People (NAACP) has consistently used social scientists in its desegregation cases, while the NAACP Legal Defense and Educational Fund, Inc. does not. (At least this is my understanding based upon statements by Mr. Melvin R. Leventhal, Staff Counsel for the NAACP Legal Defense and Educational fund, at the "National Forum on Alternatives" in Louisville, Kentucky, December 1975). Thus the parties to conflict decide if and how the social scientists will play a role. In circumstances where social scientists are called, how well have they aided the quest for justice?

The only detailed knowledge we have of the contribution of the social scientist under such circumstances is Professor Eleanor Wolf's (1976) monumental study of all social science testimony in the Federal Court's Detroit School Segregation case. My conclusion from reading Wolf is that social science testimony in this case was a disastrous failure, which served only to misinform the judge. A few more "victories" for the use of social science as Detroit, and we shall all be undone.

Perhaps we already are. I have taken one segment of Judicial conclusion from the Boston School Desegregation case, and will order my analysis from what one panel of Federal Judges stated about social science testimony presented to them; to my view, theirs is a weariness verging upon sarcasm. I have placed the full statement in Appendix A, so the reader may judge if I have misinterpreted their irony.

The reports mentioned in Appendix A begin with Professor James Coleman's analysis and then continue with rebuttal and rejoinder. In the next section, I will discuss Coleman's work, for it is his study which is the bone of contention; in a subsequent section, I will discuss most of the other papers referred to.

To preview my general conclusions, I have many criticisms of Coleman's study, but my interpretation of his research (rather than his interpretation of what are the implications for social policy) is that school segregation is high, there has been some decline but it has not been consistent or simple to describe, and in some cases has risen. Moreover, except for a small number of the very largest city districts (22 to Coleman, but little more than a dozen to my mind), there is no evidence that school desegregation is likely to cause "white flight". Moreover, even where there is an effect, it is but one-time, after which school desegregation ceases to be of consequence to "white flight". For all the pyrotechnics, I do not read his critics saying anything much different about the facts. The only major contention is over whether Coleman is correct in believing this is or could be a problem for these very largest districts. In the hope of shedding some light on this issue, my paper will end with some discussion of Boston, one of these disputed very largest districts.

I might add in closing that I will not pass any judgement upon what is obviously the fiercest of these conflicts, the judgements concerning what social policy should be pursued in view of these facts; I have already said, I know little of school organization, so I have no expert idea of which is best or worst, or what might make a desegregation plan acceptable or not.

4. THE RESEARCH OF COLEMAN, KELLY, AND MOORE

The Different Versions/Revisions

The original paper by James Coleman entitled "Recent Trends in School Integration", was given April 2, 1975 to the American Educational Research Association. This 30 page paper (including tables and graphs) along with a 4 page affidavit by Coleman, signed June 12, 1975 was submitted to the Federal Court as the material mentioned in my Appendix A. The 6 points of the affidavit identify Coleman, summarize his findings and the implications he draws from them. Points 3 and 4 of his affidavit give a succinct summary of his conclusions on segregation and white flight:

> 3. Beginning last fall, I initiated research using data collected by the Office of Civil Rights of the Department of Health Education and Welfare on the racial composition of each school in the country in the years 1968-1973. I first investigated trends in racial segregation throughout the country, distinguishing between segregation that existed between schools within the same school district, and segregation due to blacks and whites and whites residing in different school districts. I found that while segregation between different schools within the same school district was, on the average, decreasing, segregation between different school districts within the same metropolitan area was increasing nearly everywhere.
>
> 4. This finding led me to raise the question of what the specific effect of widespread desegregation within a central city school district was in increasing segregation between the central city district and the surrounding suburban school districts. I found that when widespread desegregation occurred in large cities during the period 1968-1970, there was massive loss of white children from those central city districts during the period 1970-1973. For example three of the five large cities in which the most extensive desegregation had been imposed in the period 1968-1970 were Atlanta, Memphis, and New Orleans. In these three cities, the loss of white children from the central city district in 1970-1973 was 52% in Atlanta, 43% in Memphis, and 27% in New Orleans. These were the highest losses of white students from any large central cities in the country during these years. The statistical analysis which includes other cities as well, confirms the

general result that rapid increases in loss of white children from central city schools follows immediately after rapid school desegregation. There has been some exodus of whites from central cities in the absence of desegregation, but this loss is much smaller. For example, the predicted loss in 1970-1973 for Atlanta in the absence of increased desegregation which occurred 1968-1970 would have been 17% of the whites in the system in 1970 instead of the 52% which actually occurred.

This April 2 paper was the beginning, and with affidavit of June 12, 1975, comprised the document referred to Appendix A.

The most recent report is a perfect-bound paperback monograph: James S. Coleman, Sara D. Kelly, John A. Moore, Trends in School Segregation, 1968-1973, Washington, D.C., The Urban Institute 722-03-01, August, 1975, 133 pages. The final report is forthcoming, according to the Preface by William Gorham, President of the Urban Institute, and the August, 1975 monograph "is part of a larger study...on trends in education, which will be drawn on as a chapter in a book on the quality of metropolitan life to be published in 1976" (p.iii).

This August monograph bears the marks of preparation under pressure of time, for included with my copy was an 18 page "Insert" which included 7 pages of reinterpretation and "Errata and Modified Calculations", plus 11 pages of "textual revisions" and "revised Tables and Graphs". The "Insert" mentions on p. 7 that in addition, a revised Appendix 3 is separately available.

Interim versions are alluded to by some of Coleman's critics. For example, Green and Pettigrew (1976;402) list in their footnote 7, the following dates for versions: April 2, (the paper presented to the American Educational Research Association), and "unpublished" versions of July 28, August 15, and one cited as simply "August, 1975". On p. 401, they mention a "one-day conference at the Urban Institute on August 4, 1975", which may be their second August citation. I have seen only the April 2 paper and the August, 133 page monograph, and will restrict my discussion to them.

April 2, 1975 Paper

Because this paper is not readily available, I have abstracted it in Appendix B, although my categories do not follow the organization of the paper itself. I found I had misunderstood some points in the paper when I later read the August monograph. I have left the mistakes in Appendix B as a record of my lack of understanding or of lack of clarity in the original paper; some of Coleman's critics who saw only this paper may have had similar reactions.

I will comment on the material in Appendix B in the order of my columns, where I have listed first some detail of Coleman's conclusions. The succinct summary already quoted in his Affidavit of June 12, 1975 is probably clearer in giving an overall view of his conclusions. However, it may be of interest to note that pages 1-15 concerning segregation levels and changes have not been the focus of criticism, but rather these have concentrated on the material on pages 15-20, on "white flight". The focus of the criticisms is Coleman's conclusions concerning the 19 largest districts (where he concludes there may be "white flight"), rather than those for the next larger 50 districts (where he concludes desegregation does not lead to "white flight"). Some confusion attends the fact that the number of largest districts is 22, 21, 20, or 19, and that of the others is 50 or 46, depending upon the paper, or the stage of analysis one reads.

In the column "Populations and Sources of Data", Coleman focuses on Black and White children in Elementary and Secondary schools combined. He mentions other minorities such as Spanish Surnamed children, but I did not realize until I read the August monograph and the 1972 Office of Civil Rights Report that the actual data collected included a wider range of American Indian, Oriental, Spanish Surnamed American, Negro, Other (white non-minority and undesignated minority).

More specifically relating to the April 2 paper's conclusions on p. 15-20 of "white flight", Coleman analyzed the 19 largest central city school districts, (omitting Washington, D.C.), and also the next 50 in size. I mistakenly assumed these were all districts coterminous with the central city legal boundaries themselves, and only later learned that some of these school districts could also be county-wide ones (such as Tampa) which included both the central city and the outside suburbs. Moreover, in the August monograph, he mentions a few districts restricted to the city itself had extensive annexations during the period studied. Also, the April 2 paper led me to wonder if the data included census statistics of migration; it did not, a fact clarified in the monograph.

The quality of the basic statistics raises questions. All of Coleman's work and nearly all other research in school attendance rely on statistics of school enrollment collected by the U.S. Office of Civil Rights. So far as I have been able to de-

termine, neither Coleman nor other students of these data have considered how complete or accurate are they. The result has been, to my judgement that all these writers have presumed a greater precision to their data, and hence to their conclusions, than the evidence warrants.

The basic problem of the enrollment statistics is there have been no checks built into the enumeration procedures, making possible occasional gross errors, and probably increasing the average error. My only evidence concerning completeness comes from questions put to a few local Ohio school teachers and school administrators, a telephone conversation with an official at the Office of Civil Rights, and an inspection of the prefatory material in the published data for 1972 (U.S. Department of Health, Education, and Welfare/Office of Civil Rights, 1974). The survey was actually conducted under contract by two private companies, Opportunity Systems, Inc. (Washington, D.C.) and Lambda Corporation (Arlington, Virginia). The basic unit is students by enrollment at the beginning of the school year, except for a few places, notably California, which reported Average Daily Attendance rather than Average Daily Enrollment. I suspect enrollment data are somewhat different from attendance, if truancy in slum schools is much higher than in middle class ones. Another matter of definition is that the data were collected for the following populations: American Indian, Oriental, Spanish Surnamed America, Negro, Other (White non-minority and undesignated minority). These definitions may soon give way to "Hispanic", defined as "persons who wish to be designated as Hispanic". Such a self-identification theoretically would lead to questioning every child, which seems to be an impossible task.

I have found no references to any empirical studies of enumeration completeness or classification accuracy by the collecting agencies. The basic statistics are simply compiled and forwarded to Washington by individual school districts who provide all the labor. I imagine these school districts do their best, but the general procedure seems to be that the school district headquarters send the forms to the individual school principals who then fill them out.

The sample is 100 percent of districts of 3,000 or above, with reduced sampling fractions for proportionately smaller districts, until districts of less than 300 students are excluded (in addition there are special samplings to take in such districts as those with a special segregation past). This leads to 8,056 districts on file, 46 percent of all school districts in the U.S., 72.5 percent of schools (but which contain 91.8 percent of enrolled students). This means there are a lot of school principals in these 8,056 districts filling out these forms, and no one knows how well they do.

I am sure these principals do their best, but I suspect theirs is a busy, hectic life burdened with detail and forms; some have adequate staffs, some not; some may have some central file of the data required but I imagine most do not. Under the circumstances of their daily jobs, I imagine there is a great deal of variation in their ability to state that among their few hundred or few thousand students, there are so many American Indians, Orientals, Spanish Surnamed Americans, Negroes, and Others. Also, I imagine that their school district's central administrative offices provide help, but this also must vary. The only central controls in Washington have been editing by computer for consistency (p.v):

> A frequent discrepancy occurred when the sums of the individual school data items did not equal the district totals reported on the district summary form. In order to expedite the editing process--as well as attempt to control costs--only those numerical errors representing more than 2% discrepancy from the corresponding total were flagged by the computer for editing attention. The discrepancies were brought to the attention of the school district or State agency supplying the original data and were subsequently resolved by the originating organization. The other remaining differences were "forced" to agree by the computer and entered in the file as "correct".

The end result is, I am sure, statistics which are useful but which are not likely to be exceedingly precise, especially in absolute numbers.

Another issue is the measurement of "Segregation". In the column "Variables, and/or Indexes of Segregation", I have outlined Coleman's definitions of his segregation index numbers, which are different from the most commonly used one in demographic studies of segregation, the "Index of Dissimilarity". In a long footnote in the April 2 paper he discusses the Index of Dissimilarity, and rejects it. Which index number one uses is a matter of convention and judgement, and in my own, I think I would have used the Index of Dissimilarity (as does Reynolds Farley, 1976). Since some differences on interpretation hinge on the segregation index used, and since the indexes themselves are but matters of convention, a few comments may be pertinent. Indexes have no inherent validity and are not ratio scales; like other indexes such as the Bureau of Labor Statistics Consumer Price Index which does not measure the "real" Cost of Living, segregation indexes do not measure "real" segregation. Rather they are statistical summaries which are useful for comparison.

The Index of Dissimilarity is the segregation index most commonly used by Sociologists, and thus has the advantage that studies which use it can be reasonably compared with one another. Intuitively, one could view it by considering a school district with two schools: if, say, half of all whites and half of all Blacks in the district attended each school, one would have perfect inte-

gration because 0 percent of either population would have to shift to achieve an equal distribution; at the other extreme, if all whites attended one school and all Blacks the other, 100 percent of either population would have to shift to achieve an equal distribution. The index as it is usually used is independent of the size of the total enrollment of the schools, and also of the proportion of the total enrollment which is Black. This is a limitation because it weights large schools and small schools equally, and a school district which is 2 percent Black counts equally with one which is 50 percent Black. Some have thought to weight this index by the absolute numbers of the size of the school and/or the proportion Black, but this is not unalloyed gain, for it confounds the concept of segregation which can exist independently of whether a school system is large or small, or whether it is 2 percent or 50 percent Black.

Coleman defines his index of segregation differently. Instead of asking what percentage of all the whites in an entire school district attend school A, as does the Index of Dissimilarity, Coleman asks what percentage of all the students in just School A are white. Coleman then averages these percentages for each school in the district. Moreover, Coleman weights these percentages by the absolute size of the Black enrollment in each school, arriving at a weighted average which he calls a measure of contact - the proportion of white children in the school of the average Black child. (One can also calculate the proportion of Black children in the school of the average white child). Coleman then calculates measures of "total", "within district" and "between district" segregation by dividing the measure of contact by the overall proportion of Blacks in the entire school district or the entire United States. Although I tend to be unsympathetic to Coleman's definition, his is intellectually just as "valid" as the other.

This leads to Coleman's definition of "desegregation", which is a decrease in his index of desegregation, and which does not necessarily mean that the school district had a formal desegregation plan such as a Court ordered one. I misunderstood this in the April 2 paper, but it is clarified in the August monograph. I will discuss this in more detail later in this paper.

In the column "Methods of Analysis and Evidence Presented" for pages 1-15, there were tables and graphs included, but the material on pages 15-20 was simply a summary of conclusions. The August monograph includes extensive data, but it is understandable that since the controversy focused on the material on pages 15-20 of the April 2 paper, those who took exception to his conclusions were understandably concerned to see the evidence. Another aspect of Coleman's research which I think has caused problems is whether his aim is prediction, or explanation, or both. As I will explain later, I have proceeded in the belief that Coleman's aim is prediction (or projection).

Urban Institute Paper 722-03-01, August 1975

This 133 page monograph, with its 18 page "Insert" and revised Appendix 3, comprises the current and next-to-last document before book publication in 1976. It consists of 81 pages of text and data, the remainder, Appendixes.

The first 52 pages consist of chapters headed "Introduction", "The Measures of School Integration", "Integration in 1968", "Trends in Within-System Segregation" "Changes in Total Segregation and Segregation Between Districts", and "Within-and Between-District Segregation in Metropolitan Areas". This material has not been the cause for debate. With greater detail and a number of changes, it covers the material of pages 1-15 of the April 2 paper. We will not go into detail over this part of the monograph. I might mention however that this section educated me to the problems of using the "primary" and "secondary" school dichotomy, to the point where I am skeptical that it has much meaning when comparing different cities over time.

Another aspect of this section should be covered in some detail, the classification of the largest central city school districts. In the April 2 paper, Coleman used the largest 20, less Washington, D.C. which was nearly all Black in population, or 19. In the monograph, Coleman uses the largest 22, including Washington, D.C.: New York, Los Angeles, Chicago, Philadelphia, Detroit, Houston, Baltimore, Dallas, Cleveland, Washington, D.C., Memphis, Milwaukee, San Diego, Columbus, Tampa, St. Louis, New Orleans, Indianapolis, Boston, Atlanta, Denver, San Francisco. In his analysis he drops Washington, D.C. again, making it 21 districts. He reports his basic list of cities comes from a roster defined by the U.S. Office of Education.

Terms which should be specified are: what is "large", and what is "central city". (p.23):

> These 22 largest central city school districts are classified according to 1972 enrollment and an Office of Education metropolitan status classification. They represent 22 of the 23 largest central city districts; Albuquerque is excluded (the 22 largest) because it is not among the largest 50 cities in total population.

From Appendix 3, the 1972 enrollments for all grades in these largest ranged from 1,125,449 in New York City to 81,970 in San Francisco (Washington, D.C. is missing from Appendix 3). The remaining cities range from 79 813 in Charlotte-Mecklenburg to 35,853 in Colorado Springs. The particular cutting point for dividing "large" from the remainder (or restricting the analysis to the "largest" 70 central city school districts) is a matter of judgement, based on the assumption that there is qualitative difference between "large" and "not so-large".

A more vexatious problem is the definition of "central city"

school district. I thought the April 2 paper prescribed only districts such as Boston which are co-terminous with city boundaries. However, the monograph clarified the definition, for Tampa, a countrywide system is included because it is a central city. In addition, on p.72 (added from the "Insert"), Coleman refers to a letter from Reynolds Farley that Houston, Dallas, Memphis, and Denver annexed substantial territory during 1970-1973. School systems which are county wide or have had considerable annexation, make it more difficult, I think, to find white flight caused by residential moves: a school system such as Boston which is co-terminous with the central city and where the Census Bureau's Standard Metropolitan Statistical Area (SMSA), shows only about 1/5th of the total population lives in the central City is a far cry from the countywide district of Tampa. In Boston one may move to a nearby suburb; in Tampa, one has to move to another metropolis.

On the other hand, Green and Pettigrew (1976) believe that Coleman biases his sample in favor of finding white flight by including Memphis and Atlanta ("two atypical cities of the Deep South") as well as Denver and San Francisco ("Which he added late in his analysis"). Beyond these cryptic remarks, I have no way to judge the merits of either Coleman, or Green and Pettigrew, with regard to these cities. Obviously, judgement is involved. For example, Green and Pettigrew (1976) report they contradict Coleman when they replicate his analysis with all his "original" cities plus additional districts they felt should have been included: Miami, Jacksonville, Fort Lauderdale, and Nashville, all countywide systems; plus Denver and San Francisco (I gather they were working from one of the earlier version of Coleman's papers because Denver and San Francisco are included among his 22 in the August monograph); plus Albuquerque, Charlotte, Newark, Cincinnati, and Seattle. They state they used a standard cut-off of 75,000 students. I do not know any objective resolution to such a conflict; each side has its own definitions and assumptions. However, I hope that some of my later interpretation will provide some guidelines for judging the alternatives.

The remainder of Coleman's text consists of the Chapter "The Size of Individual Segregating Responses to Desegregation" (p. 53-81), where the controversy lies. This chapter is a multiple regression analysis of public school enrollment statistics 1968-1973 in which the independent variables are for the District as a whole, and year by year, changes in the Index of Segregation, Blacks as a proportion of all students, total students; the dependent variable is change in numbers of whites as a proportion of all whites in one previous year. In addition, dummy variables are introduced to consider factors such as the South versus all other regions; the 22 largest districts are analyzed separately from the 50 smaller; and an analysis of covariance is carried out to estimate loss rates for each city which underwent substantial desegregation.

If I may briefly review this chapter, commenting upon selected parts, Coleman begins by stressing it is concerned with causation (p.53):

> What is not yet clear is whether desegregation itself induces an increased movement of whites from the desegregated district. This is a difficult but important question to answer, because desegregation in particular school districts is a direct outcome of social policy or legal rulings, and it is important to ask whether there are indirect consequences of desegregation itself which partly nullify it, and if so, what the size of this response is under various circumstances.

This quote is important both as a specification, and also as the basis of 2 confusions. The first confusion is his definition of cause and effect, which I view as "prediction" rather than "explanation". Thus those of his critics such as Rossell or Wisenbaker who object to the fact that Coleman does not use U.S. Census demographic variables, or that he does not create an ecological model of population succession in the city attack him, I think, for something he never intended to do in his prediction/projection approach. Thus, of his class of the largest 22 districts for which he projects white flight, he is saying that a (unspecified) large number, far more than a majority of these districts will experience white flight upon "large scale" desegregation. A similar but reverse argument holds for Coleman's smaller districts. Secondly, he also lays the basis for another confusion by his definition of "desegregation". Most readers, I think, reading above, that "desegregation in particular school districts is a direct outcome of social policy or legal rulings", would infer that Coleman's definition of "desegregation" is limited to formal, deliberate school board policy, or rulings by courts. Not so. On p. 58, he gives his operational definition of desegregation:

> Note that the independent variable measuring change in segregation $\Delta r_{t,t-1}$, is just that. It is not a measure of a particular form of change in segregation, such as bussing, nor even of a desegregation policy. Change in r can occur through individual movement of black or white students; and certainly the slight upward movement of segregation (as measured by r) in some northern cities is just that. However, these individual movements make only small differences in r over any year. Large negative values for Δ r are due to segregation policies instituted in that city. Although the term "desegregation" to a civil rights lawyer may mean only the move to full racial balance in all schools, it is important to remember that the desegregation variable used in this analysis refers to a reduction of any size in the index of segregation.

This definition is a critical point which some readers may not have understood: desegregation is a change in Coleman's Index of Segregation. But Coleman states as a matter of historical fact, the Index has changed drastically ("large negative values") among the 72 school districts studied only when there was a formal governmental plan, whether court imposed or not. Unfortunately he gives no details.

I am not satisfied with Coleman's definition, but he has some reason on his side; just because a formal plan is officially implemented, does not necessarily mean that the end result will be less "segregation". By linking his operational definition to his index number he circumvents the problem, at least in part. However, he is negligent for never defining what he means by a "segregation policy instituted in that city". For example (p. 55-56) he explains he includes Denver and San Francisco in his final analysis because they "were two of the few northern cities to undergo extensive desegregation during the period of 1968-73". He excludes Washington, D.C. from much of the analysis because it was 97 percent Black; his small group of cities included 46 of the next 47 largest central city districts, excluding Richmond, Virginia because of extensive annexation of suburban districts.

Coleman also makes reference to other studies concerning white flight, among them Cataldo, et.al. (1975), an attitudinal study of 8 countrywide Florida systems; Mercer and Stout's, study (cited in our Appendix A) in which they found no white flight for California school districts; Clotfelter (1975) who did find white flight in Mississippi school districts; and Farley (1975) who did not find white flight in a very extensive national analysis of U.S. cities. Farley's work is extensively cited by critics of Coleman, so we will consider it separately. With regard to Farley's work, it might be mentioned that both Farley and Coleman point out that they disagree in conclusions, but neither carry out an analysis to inquire why and to what significance. For example, Coleman's entire explanation is the laconic comment "His methods differ, however, from our own in several respects, particularly in our year-by-year examination contrasted to his five-year examination". I will try to discuss this difference later.

Coleman next has a discussion of some problems and judgements: the South differs from the North; more desegregation took place in the South; large districts qualitatively differ from small ones; the data are restricted to school enrollments (later in his analysis, e.g. revised table 14, it seems to me that he used school enrollments within census bureau Standard Metropolitan Statistical Areas as units to define inter-district segregation); long term effects may be different from short term ones.

We may now turn to ask what does Coleman conclude? Here the regression approach shows its value in enabling us to consider alternative assumptions (p. 60-61):

1. For a city with the average number of students, with no blacks and no reduction in segregation, the expected loss per year is:
   a) Largest 21: (gain of) 0.9% of whites present at beginning of year (average number of students is 169,000)
   b) Next 46: 1.2% of whites present at beginning of year (average number of students is 58,000)

2. Additional expected loss if the city is 50% black:
   a) Largest 21: 6.8% of whites present at beginning of year
   b) Next 46: 4.5% of whites present at beginning of year

3. Additional expected loss if the city experiences a decrease of .2 in the index of segregation in that year:[25]
   a) Largest 21: 5.6% of whites at beginning of year
   b) Next 46: 1.1% of whites at beginning of year

4. Additional expected loss if a city was twice its size:
   a) Largest 21: 0% of whites present at beginning of year
   b) Next 46: 2.9% of whites present at beginning of year

Taking the first three losses together, the expected loss of whites from a city system with 50% blacks would be:

For the largest 21:

with reduction of .2 in segregation: (-) 0.9% + 6.8% + 5.6% = 11.5%
with no change in segregation: (-) 0.9% + 6.8% = 5.9%

For the next 46:

with reduction of .2 in segregation: 1.2% + 4.5% + 1.1% = 6.8%
with no change in segregation: 1.2% + 4.5% = 5.7%

These are Coleman's most general results, and they project that white flight for the largest 21 districts will nearly double from 5.9 percent with no change in segregation, to 11.5 percent with a "large" reduction in segregation. On the other hand, he concludes that among the smaller districts, desegregation has virtually no effect on white flight: a 5.7 percent loss with no

25 A decrease of .2 in the index of segregation is approximately equal to an increase of 10% in the black schoolmates of the average white in the system if the proportion is .50.

change in segregation, versus a 6.8 percent loss with a "large" reduction in segregation. Of course, a .2 decrease in segregation as the definition of "large" is itself a matter of judgement, as is my conclusion that the increase in white flight is important for the largest districts (5.9 to 11.5 percent), but not so for the smaller ones (5.7 to 6.8 percent). But withal, only among the 21 largest districts does he project that desegregation may cause appreciable white flight. In this, we can see a great deal of similarity between Coleman and the critics who claim that desegregation does not cause white flight, for Coleman concludes that we may expect it only in the very largest 21 of all districts.

In the next section (p. 62-72), Coleman wishes to account for such factors as region (South vs. North); the degree to which suburban alternatives are available (a section I found very difficult to understand); and among the largest districts, to isolate unique aspects of each district by using white student loss during 1968-69 as independent variable to predict 1969-1973 losses. This section concludes that "large" reductions in segregation in his 21 largest districts cause much more additional white flight in the South (6.8 percent) than in the north (4.0 percent); in smaller districts, a small amount in the South (1.9 percent), with no estimate possible in the North.

Furthermore, higher proportions Black, combined with high "Between District" segregation is associated with greater white flight in the South, particularly in the largest districts; in the North, even in the largest districts this white flight is much less, with no statistical estimate possible at all for smaller northern districts.

Finally, on pages 73-77, Coleman projects the consequences of "large" reductions in segregation for 10 years, and concludes the long term effect is virtually nil. As a hypothetical example he uses a large northern metropolis with an inner city district 50 percent white and an equal sized suburban ring school district 100 percent white. The Black percentage at the end of 10 years without desegregation would be 65 percent; with substantial desegregation, it would be 75 percent; hardly much of a difference.

Coleman ends by emphasizing the variation inherent in his projections once we go from hypothetical to actual districts. For example, he states (p. 76-77) that in such districts as Detroit, Baltimore, Philadelphia or Chicago:

> the impact of full-scale segregation would be ... very large, moving the city's schools to nearly all black in a single year. What would happen in a particular city is unknown; the point here is that the white loss depends very much on the extent of desegregation, the proportion black in the central city and the black-white differential between central city and suburb.

I conclude that Coleman, Kelley and Moore have made some very telling points about "white flight", even when we allow for differences in judgement, as well as limitations of data and method. They conclude that massive desegregation has little or no effect on "white flight", except among the largest 21 central city school districts, and even there, much variation exists which depends upon such factors as region, percentage Black, city-suburb differences and of course, how "large" is a "large" scale reduction in segregation. Moreover, even when one does have conditions of great "white flight" in these largest districts, the net result over a decade is minimal, a reasonable projection in light of the longterm steady decline in the white and middle class populations of some of these central cities. All in all, these seem to me moderate conclusions. Also, if we deduct from these 21 districts those where desegregation has already occurred (depending on one's definition), we are left with little more than a dozen which are strong candidates for white flight. Among these remaining, many like Boston were only hypothetical projections during the 1968-1973 period of Coleman's data, and like any hypothetical projections only just may, or just may not be tokens of the future. But if we are concerned to minimize white flight as has developed in Boston, we ignore such tokens at our peril.

Summary

A violent debate was precipitated by Coleman's 20 page paper given April 2, 1975 to the American Educational Research Association, centered about the last 5 pages which presented his conclusions concerning "white flight". Unfortunately, these 5 pages included no evidence. During the following 5 months, I have seen allusions to three more "versions" of the study; finally, in August, a 133 page monograph appeared, with its own booklet of errata and revisions. Different writers sometimes allude to different versions, which makes for occasional confusion.

If I may restrict myself mainly to the August monograph, Coleman concludes there has been some reduction in school segregation within public school districts during 1968-1973, but there was generally increased segregation between school districts. These conclusions have aroused no controversy.

The controversy swirls about the remaining conclusions of "white flight" as a consequence of school desegregation. Coleman considers the largest 22 (or 21) public school districts separately from the next 46. Among the 46 (and by implication, even smaller ones), Coleman projects that school desegregation does not generally lead to "white flight". However, among the largest 21, he does; I suspect many who have occasionally read about the controversy have not realized that the conflict is over just these 21 districts. Moreover, when I deduct those districts, such as Atlanta, which evidently have already had "white flight", or Tampa which have not, we are left with perhaps a dozen districts in which the issue is in doubt. To my view, Coleman has rendered a great service by pointing out that there is a high probability of school desegregation leading to "white flight" in these dozen cities. He also concludes that even in the event of white flight, this tends to be a one-time effect, so that after about a decade the percentage white in a school system would differ little whether there had been desegregation or not. This could be evaluated from two points of view. One way would be to conclude that school desegregation, even where it does lead to white flight, creates only a minor acceleration to the perhaps inevitable trend. An alternative view which Coleman espouses, is that such school desegregation is counter-productive because it accelerates what it was designed to prevent.

Aside from the battles royale and ad hominem which I ignore, there are a number of technical issues which have caused confusion. Coleman defines "desegregation" as a reduction in his index of desegregation, not as some think, a formal court-ordered plan; his definition of cause and effect is a predictive/projective one growing out of a multiple linear regression analysis, which has created something of an hermeneutical debate between him and his critics. Finally, Coleman never discusses the quality of his basic evidence; unfortunately, the enrollment statistics he uses are unlikely to be as precise as he seems to think they are. In all fairness to Coleman, I would make the same criticism of others who have used these data.

5. COLEMAN AND HIS CRITICS

There are a number of recurring issues in the criticism of Coleman, some of which we omit, such as the ad hominem, the debate in the mass media, and that over what should be proper public policy with regard to schools. We shall be concerned only with the empirical issue of white flight as the focal point of residential segregation of whites and blacks from one another as a consequence of school desegregation. An unfortunate omission will be the distinction between whites moving residence (and so leaving the local school system), or withdrawing from the public school system (with or without a residential move) into a private school. We shall allude to the difference between the two in a later section, but because white flight in the current debate concerns itself solely with white students leaving a public school system for either reason, for the present we shall discuss the debate on its own terms: and of course, there is no consideration of white ethnic or black separatism in the debate, so it is set in terms of racial prejudice only.

As we shall try to make clear in the following pages, most of the disagreement about empirical fact between Coleman and his critics is more apparant than real. Because we shall discuss each author in turn, perhaps it will be best to outline the recurrent themes. First is the fact that in many broad areas of empirical conclusions, there is no disagreement; for example, except for the largest 22 public school districts, everyone agrees that as a general rule, school desegregation causes little discernable white flight. Second, when one looks at the actual disagreements over these 22 districts themselves, one finds such incredible differences in definitions, one is hard put to make any sense out of them; for example, virtually none of the participants operationally define "desegregation" the same way. Third, there is great divergence in the implied meanings which the participants give to the concept of cause and effect. Coleman uses a simple prediction/projection definition in which a few independent variables effect, in a multiple linear regression model, the dependent variable of loss (or ocassionally, gain) in white students; some of his critics begin from a more complex explanatory model which involves the ecology of the city, or statistics from the decennial census. Fourth, some of the participants argue over their statistical approach; for example, some claim there are better ways than multiple regression, others say not. I hope to present sufficient evidence for the reader to be informed, but I myself do not see any merit in any of the claims to greater validity of one approach to another. My own view is we are faced with a practical issue of projecting possible change for use in public policy for about 22 school districts, as many as half of which may already have gone through some sort of extensive desegregation (depending upon how one defined desegregation). I think a more intensive investigation of just these 22 would have been more productive than the freeewheeling farrago we have.

It should be mentioned, also that there have been methodological storms of controversy which have swirled around this chapter. I shall not comment extensively upon these issues for two reasons. The first is that while I believe I am competent to follow most of the arguments, I know I am not technically competent to authoritatively judge each

of them in detail. More important, I cannot do what the various critics do not, that is detail exactly which of Coleman's results and conclusions would be changed to what extent if their criticisms are valid. For example (Green and Pettigrew, 1976:401) state:

> We would take issue with Coleman on another important methodological point. In his second analysis, in a paper titled "Trends in School Segregation, 1968-1973", presented at a one-day conference at the Urban Institute on August 4, 1975, Coleman offered a second set of prediction equations. It demonstrated that when three more predictors were inserted, 60% of the variance in changes in percentage of white population could be predicted by the new eight-variable equation. This was accomplished largely by the addition of the dummy variable for the South and the interactions of change in desegregation with both interdistrict metropolitan segregation (ΔR x RSMSA) and the black proportion of students (ΔR x Prop. Black). But the point to be observed here is that the increase in predictive power is due to interaction effects, rather than being a main effect of desegregation per se. Whether desegregation will produce any discernible effect on white flight appears to be conditional upon other factors such as the percentage of black children in large central-city districts. This is an important part of his data which Coleman has not emphasized. Unfortunately, public recognition of his work usually focusses on the simple effect of desegregation on white flight.

So far as I think I understand Green and Pettigrew's points here, their objections are reasonable. But I would also need Coleman's rejoinder; there are literally dozens of points such as this. Finally, when I consider that Green and Pettigrew end by stating in the above "This is an important part of his data which Coleman has not emphasized", I have no means of knowing whether they mean all of part of Coleman's work is worthless, or whether some specific point projections should be changed to some specific extent. See also Wisenbaker (1975).

I view Coleman's results skeptically, but no more so than any other empirical research, for there are always points of diverse judgements. For better or worse, until the cognoscenti can show specific or consistent distortions in Coleman's results which lead to erroneous conclusions, I consider these criticisms as differences in judgement.

In Appendix A, the first rebuttal to Coleman is that of Professor Jane Mercer. I have not been able to obtain copies of affidavits or testimony, but Professor Mercer kindly sent me a copy of the 35 page unpublished paper (Mercer and Stout, 1974) which I assume was the basis for her affidavit. This detailed analysis of school desegregation of 90 California school districts between 1966-1973 was written in 1974, so it contains no reference to Coleman's 1975 paper. Limiting myself to these aspects which refer to "white flight" and might be relevent to Coleman, much of Mercer and Stout's paper cannot be compared. But where they may, they seem not to differ.

Basically, Professors Mercer and Stout conclude there is no "white flight" associated with school desegregation. Since Coleman projected "white flight" only in the largest 22 of his school districts, his conclusions for the smaller ones coincide with theirs, for Mercer and Stout's districts were, with 2 exceptions, equivalent to Coleman's smaller ones. They included all California districts of 2500 students or more with the exception of Los Angeles, which they excluded because it was atypically large. But Coleman's largest 22 included Los Angeles, San Diego, and San Francisco, so there is an overlap of only 2 districts in 22 for Coleman and 2 in 90 for Mercer and Stout.

There are a number of other points of difference but we need only refer to the fact the basic definitions of "desegregation" differ so radically as to make impossible any close comparison. We have already discussed Coleman's definition. Mercer and Stout's hinged on whether a school was "balanced" (p.5)"

> A "balanced" school was one in which the proportion of pupils of each racial or ethnic group enrolled inthe school was within 15 percent of the proportion of pupils in that racial or ethnic group who were enrolled in schools in the district as as whole.

Mercer and Stout then develop a classification of "desegregation" based upon how many schools in the district moved away from or toward "balance", a definition so completely different from Coleman's that I cannot see much basis for comparison.

I have not see the actual material referred to in Appendix A by Dr. Weinberg, and Professors Green and Pettigrew. However, Dr. Weinberg informs me that the gist of the material is in his "A Critique of Coleman" article in Integrated Education for September/October 1975. This article touches on a number of items, including newspaper interviews with Coleman. With regard to Coleman's research in "white flight", Weinberg refers only to Coleman's April 2 paper, because Weinberg's article is a reprint of a paper given early in the debate, July 3, 1975, to the National Education Association. Quite naturally, there are many criticisms of the April 2 paper which were clarified in the later monograph, such as Coleman's definition of "desegregation". Weinberg also discusses previous research in desegregation, pointing out that this was omitted by Coleman in his April 2 paper. Professor Green informs me that much of the material in his court submitted critique is in his Green and Pettigrew paper (1976) discussed previously, and in his paper published in the Feb. 1976 issue of the Harvard Education Review (unavailable at this writing). By the time of revision of this paper, however, I have seen in the February and May HER the latest engagement between Coleman, Green, and Pettigrew.

The paper by Professor Christine H. Rossell (1975a) mentioned in Appendix A, was prepared for delivery at the Annual Meeting of the American Political Science Association. It consists of 57 pages of manuscript text plus unnumbered, extensive tables, figures and footnotes. The paper focusses on school desegregation in relation to voting behavior (p. 5-47) and "white flight" (p. 48-56). Its "white flight" section,

published in Integrated Education, (1975b), refers to only the April 2 paper by Coleman. Finally, her "School Desegregation and White Flight" article (1976) which is basically the Integrated Education article with methodological additons, refers to Coleman's April 2, 1975, paper plus an August 1975 Urban Institute Working Paper.

If we focus just on "white flight", we find essentially the same material in all three documents. Rossell states she disagrees with Coleman for she finds no relationship between public school desegregation and public school white flight, irrespective of size of school district. As we have already stated, this is in agreement with Coleman's findings for school districts smaller than his largest 22. Rossell strongly emphasizes her disagreements with Coleman. For example, in the 1976 Political Science Quarterly article (p. 687-688):

> Furthermore, in a careful reveiw of the issues and policy implications of white-flight research, Orfield points out that even if one were to accept both the validity of Coleman's method of analysis and the maximum force of his results, his study suggests only that the initiation of desegregation in a city with a 50 percent black enrollment will produce an additional loss of 5.5 percent of the white students. This "flight" is significantly less than the same school system can expect to lose for other reasons in a normal year. In short, it seems that Coleman himself has not followed his research to its logical conclusion --that at worst, his model predicts desegregation of a 50 percent black school system would bring the schools to their ultimate ghetto status only about a year sooner than otherwise projected. Therefore, an even greater problem than the inappropriate methodology used by Coleman is his apparent unwillingness to accept the policy implications of his own research.

As we mentioned earlier, Coleman (p. 73-77) does go into this aspect, although he may have not in the paper that Rossell refers to. But Rossell's quote above does illustrate that the terms of the debate include for the protagonists, not only my interest in the evidence for "white flight" and segregation, but also questions of social policy over school plans, and the accuracy and probity of writings and interviews in journals for laymen and administrators, as well as the mass media. As I have already mentioned my aim is to evaluate the evidence concerning "white flight" rather than whether the inferences based on this evidence are correct. I hope that in doing so, I have not distorted the context of the original authors themselves. I expect that Rossell may find my accounting not acceptable, for both in her original paper (p. 55-56) and the one in Integrated Education (p.9) she noted:

> The reason I have included so much data in this report is to keep the research findings from becoming a "disagreement between social scientists." What is presented is a disagreement between real data from the real world and Coleman's findings.

I'm afraid I disagree, for in my view, the differing methodologies used by the protagonists in this debate are essentially but strategies

to create analytic abstractions for comparative purposes. As I shall try to indicate, Rossell's strategies are different from, but are intellectually no more valid (nor any less so) than Coleman's. I personally prefer Rossell's approach to Coleman's, but I see no way in which it is inherently more valid than his. Moreover if Coleman's definition of causation turns on the pragmatics of projection/prediction for use in public policy, rather than the Rossell's intellectual issues of explanation, a primary basis for selection is utility, wherein Coleman does have his points.

There are a number of other differences between Rossell and Coleman. First, Rossell excludes (so far as I can tell from her tables) Southern states (Virginia, North and South Carolina, Georgia, Florida, Tennessee, Alabama, Mississippi, Arkansas, Louisiana, Texas), plus, perhaps Hawaii and Alaska. Rossell's sample of these 86 Northern districts is a preliminary analysis of a larger 113 district study she is conducting. Rossell gives no explicit basis for selecting her sample, but I guess it includes most larger school districts. I see no major conflict between Rossell and Coleman except among Coleman's largest 22 districts.

Among these 22, there is an overlap for only 13 districts (Rossell excludes Houston, Baltimore, Dallas, Memphis, Columbus, Tampa, St. Louis, New Orleans, Atlanta). But even among the 13 which they both include, we are hard put to make comparisons.

For one thing, her definition of desegregation is different, as indeed are all the ones used by Coleman, Farley, or Mercer. Rossell's definition involves students reassigned (1976:679-680):

> This study attempts to overcome the limitations of measures of school desegregation commonly used in the past, by defining "school desegregation" as the reassignment of black or white students by a local governmental body or court for the purposes of school integration. Data were collected by means of a mail questionnaire that listed the biracial schools (defined as a minimum of 10 percent black and 10 percent white) in a district and asked administrators to indicate the reason for their biracialness and the approximate date of any action taken to adjust racial balance. The measures of school desegregation policy were computed as follows: the number of black and white students in a school in the year in which an action was taken was subtracted from the number in the same school during the preceding year. The difference was attributed to administrative action if it increased racial integration. The number of black and white students reassigned in this way in each school was totaled for the school district. This was then standardized by converting the raw number of blacks and whites reassigned into the percentage of the total school district black population that was reasssigned and the percentage of the total school district white population "reverse integrated" (sent to predominantly black or formerly black schools). These figures (percentage blacks and percentage whites reassigned) were added together to comprise an index measuring school desegregation for each year from 1963-1964 through 1972-1973.

As we point out with Coleman's Index of Desegregation, the making of segregation indexes is a decision to use some convention; there are

no inherently more or less "valid" ones, and the making of such indexes seems to be an academic cottage industry. So far as I can see, there is little relationship between Coleman's and Rossell's index.

Another major problem in comparison is that Coleman and Rossell use different dependent variables for "white flight". Coleman uses the numbers of whites only, so white flight becomes the decrease in white enrollments from year one to year two (expressed as a percentage of white enrollment in year one). Rossell on the other hand uses whites as a percentage of total enrollment; a decrease in this percentage reflects white flight. To my mind neither is inherently more valid, and I would expect a comprehensive study might use and compare both. The reader may make his own choice. I prefer Coleman's definition for it is simpler, and has the benefit that its definition includes only the white population; that is, "white flight" comprises whites who leave. But whichever one chooses, the fact remains that they do not necessarily give the same results. For example, Rossell (1976:676) singles out two cities which both she and Coleman agree had extensive desegregation, Denver and San Francisco. If we compare white flight for October to October, 1968-69, 1969-70, 1970-71, and 1971-72 (where both authors present some data) Rossell reports Denver as -1.5,-2.4,-1.4,-2.0. Coleman's percentages are -2.3,-2.4,-5.4,-6.6. Rossell would conclude there was no white flight, while Coleman would conclude there was. Also, Rossell reports San Francisco's white flight as -4.1, -0.2, -3.0, -2.1; Coleman's percentages are -11.4, -1.9, -15.6, -8.0. There are but two examples, but I think they fairly show that there is some, but not much similarily between the two author's indexes of white flight.

Rossell then carries out a longitudinal analysis of annual white flight for each of her school districts for 1967 (and in some cases earlier) through 1972, analyzing for a break in the average trend in white flight related to the advent of a formal school desegregation plan in a specific city in a given year. I find this strategy of analysis more interesting than Coleman's multiple regression approach, but there is hardly any basis for considering it the only (or even the more) valid one. Furthermore, I find no unique validity to either her desegregation index of her index of "white flight". Finally, when we inspect Rossell's 13 school districts which coincide with Coleman's largest 22, and find results such as that mentioned above for Denver, I see no reason to prefer either author.

Coleman, in his affidavit to the court (as mentioned in Appendix A), refers briefly to these differences between himself and Rossell, adding such others as the fact that his data are one year more recent (to 1973). As we might expect, however, his conclusion is not that his conclusions are different, but rather that his are better (just as Rossell has argued that hers are better).

At this point the reader might conclude, as did the Federal judges, that methodology was the last refuge of the pedant. However, I think it is fair to also point out that Coleman has something valuable to contribute if we judge it on the pragmatic basis of prediction rather than on Rossell's intellectually more rigorous explanatory model. In

various places in his affidavits to the court he refers to the kind of approach he uses in the sections (pages 69-72) of his monograph where he has taken cognizance of the approach espoused by Rossell, only within the framework of his own regression model (Coleman, 1975:69):

> There is another more stringent test of segregating effects of school desegregation than those we have examined so far. Each city, with its own particular housing patterns, suburban configurations, crime levels, distribution of racial prejudices, industrial growth or decline, and other factors, has rates of white loss that are specific to it. A rough test of this sort can be carried out for the largest cities by using the white student loss that occurred in each city in 1968-69, before much desegregation occurred in any of these cities (except for Denver), and observing what occurred from 1969 to 1973. For the twelve districts of the 22 which did not experience a reduction of at least 0.1 in segregation over the period 1968-1973 (and on the average experienced no change at all), loss of white students expected between 1969 and 1973, based on their 1968-69 losses, was 17% of the white students present in 1969. The actual loss during this period was 20%, only slightly greater than expected. For the ten districts which did experience desegregation of 0.1 or more, their expected loss between 1969 and 1973, based on the 1968-69 before desegregation losses, was only 10%. But their actual 1969-73 losses averaged 26% of the white students present in 1969.

This is essentially a justification by prediction. If we inspect Coleman's Table 16 (p.70) which lists the individual 22 districts, we find only 12 which had little or no desegregation (by Coleman's definition) during 1969-1973: New York, Los Angeles, Chicago, Philadelophia, Detroit, Baltimore, Cleveland, Washington, Milwaukee, Columbus, St. Louis, Boston. If we omit Washington which no longer has white children in its public schools, we can say that Coleman is making a projection for these remaining 11 districts; we can expect that a "large" reduction in segregation will in all likelihood lead to "white flight". In the nature of such a projection, no one can really predict the future--that is, say each and every one of these districts must follow this rule, but we may expect it. In my view, this projection for these 11 cities is the only valid bone of contention between Coleman and his critics. (Perhaps I underestimate: Add a half-dozen and we have 17 districts.) And in fact, two of these 11 have undergone court-ordered desegregation program since these data were available, in Detroit and Boston. I know little about Detroit, but we have had enough actual experience with massive school desegregation in Boston to see that 'white flight' has indeed materialized. None of this is to say that demographic change elsewhere stops; those districts which were in the process of losing whites may be expected to continue to do so irrespective of the affect of the schools. Also, of course, there will be some smaller school districts which will see white flight upon the advent of large scale formal school integration, but Coleman (and his critics) all agree that by and large these will be the exceptions.

I would like to conclude with a short discussion of the research of Reynolds Farley on school desegregation and white flight. Although this goes beyond my self-imposed guidelines of Appendix A, Farley's

study is often cited as comprehensive empirical contradiction of Coleman, both by Coleman and by his critics, and it behooves me to explain where I think some of the divergence lies.

Professor Reynolds Farley has been carrying out large scale studies in school and residential segregation for several years, and in 1975 presented a paper which was published in Social Policy (1976), entitled "Is Coleman Right". I assume this is the research to which Coleman and his critics refer when they cite Farley's unpublished research.

As I have indicated before, I would not agree with Farley's assessment that (p. 17) "(these) results tend to conflict with the body of Coleman's recent work on the subject," because both Farley and Coleman had similar results except for Coleman's analysis of the 22 largest districts. Farley analyzed school enrollment and segregation in what appears to be the largest 20 Southern school districts, the largest 20 Northern districts, and 125 cities of 100,000 or more. As we have seen, this is a different sample than that chosen by Coleman. Further, Farley uses a different segregation index than Coleman, the Index of Dissimilarity, which is the measure most commonly used by researchers in the field. However, both use the same dependent variable, the percentage change in white enrollment.

There are a number of other differences between the two, some of which are explained by Farley (p.17):

> (Coleman) analyzed data for white students while this article presents information for all non-Black students. Coleman's data pertain to all grade levels while here we discuss enrollment trends at the elementary level. Another important reason for the difference may be the emphasis Coleman placed upon observations for a few cities which experienced desegregation, particularly Atlanta and Memphis. In both places integration plans went into effect between 1972 and 1973 and these two cities account for a large share of the aggregate desegregation considered in his analysis.

Also, there are other differences. Farley measured just two points in time, 1967 and 1972 (rather than Coleman's different initial as well as terminal years, plus yearly measures of 1968-1973) and then plotted scattergrams and regression lines of his dependent variable (changes in white enrollment), first against changes 1967 to 1972 in his desegregation index, and then against the proportion of students black in 1967. Some scattergrams included the largest 20 northern or 20 southern districts, and 50 southern or 75 northern ones. We can see from this that Farley carried out a regression analysis (with the same implied predictive definition of causality as Coleman) but a less elaborate one. Farley's is a zero order linear analysis rather than Coleman's multiple linear one.

Under such circumstances, what are we to make of the often cited fact (by everyone, including Coleman and Farley) that Farley contradicts Coleman? Not much, and even less when we consider that Coleman claimed likely white flight with potential integration only for the largest 22 of his districts. Given such divergences in sample, index of segregation, years studied, zero order versus multiple linear regression methods, as well as other differences, one can hardly get excited by the fact that Coleman shows no change except for the 22 largest, while Farley's scattergrams indicate only there is some trend

of 1967 percent black versus 1967-72 change in whites for 50 southern districts.

In an attempt to gain some further insight, I used the two authors' data to plot four scattergrams for Coleman's 22 largest districts (from Coleman's Table 16 and revised appendix 3 for all students, and Farley's Table 1). This impressionistic inspection is no substitute for a detailed statistical comparison, but even so superficial an analysis suggests how their statistical manipulations lead them apart. A scattergram of Coleman's versus Farley's Indexes of Segregation for the year 1972 shows a striking correlation between the two index numbers at this one point; but when we plot a scattergram of changes in the segregation indexes (Farley, 1967-1972 vs. Coleman, 1968-1973) we see the correlation drop precipitously. In effect, the static index numbers are highly correlated, but their measurement of change is much less so.

If we next look at Farley's 1967-72 segregation change versus white enrollment loss, the scattergram shows no relationship. A similar scattergram of Coleman's reduction in segregation 1968 to 1973 versus white loss 1969-1973 is some, but not much better; but if we exclude Tampa, it is strikingly better.

We do not mean the above to be taken as any more than it is, superficial speculation based on a few scattergrams. But considering both Coleman's and Farley's laconic acceptance of their contradiction, we think it is worthwhile to suggest the thought that their segregation indexes reflect similar but not equivalent phenomena, and when one adds all the other differences, it is not at all surprising they do not concur. I see no basis for calling their differences empirically based ones.

Summary

Given my conclusion that Coleman projects "white flight" only for the 22 largest districts, the disagreement over fact should focus here. But when we look for such a focus, we find such divergence of definition, that comparability is difficult; desegregation is defined differently, cause and effect is defined differently. Some decry Coleman's use of multiple regression; others believe he has carried it out poorly. In effect, the disagreements seem to be not over matters of evidence and logic, but over assumptions and definitions.

6. SCHOOL DESEGREGATION MAY LEAD TO LESSENED RESIDENTIAL SEGREGATION

Finally, there is a view that if schools are racially desegregated, this will lead to lessened residential segregation. Perhaps such a plan may be possible, but I know of no evidence that it has ever occurred in a city. There are occasional examples of some people drawn to an area because of racial integration in the schools, but so far as I know, these are isolated examples such as Wolf and Lebeaux's discussion of the small (but unspecified) numbers of white families who moved to a middle-class central city enclave in Detroit for this reason.

Although this view has no empirical support, it is important for two reasons; first, it is held by some social scientists, and second, it appears to have been accepted as fact in at least one school integration dispute. Among those social scientists who hold to this view, is Professor Karl Taeuber (1975:842-844). Although his last sentence is his conclusion, I have quoted extensively to reproduce his entire argument:

> Much of the discussion of the connection between school segregation and residential segregation has proceeded on the assumption that the stimulus goes in one direction only. This approach gives temporal priority to residential patterns. Thus, where assignments of pupils to schools are based upon residential proximity, the racial composition of neighborhoods directly and obviously affects the racial composition of schools. Schools that serve predominantly white neighborhoods will be predominantly white; schools that serve racially mixed neighborhoods will have pupils of both races, and schools that serve predominantly black neighborhoods will be predominately black.
>
> However, the cause-and-effect relationship between residential and school segregation can be viewed the other way as well. The racial composition of a school and its staff affects the racial composition of housing within the school's attendance zone. This is especially apparent in neighborhoods experiencing racial change. The changing racial composition of the school's pupils and staff serves as a signal to the public - realtors, homeseekers, residents, etc. - that school authorities expect the school to become all black. Families with children, and everyone else, too, may use school attendance zone lines to make their housing decisions.
>
> Several factors contribute to the influence school attendance zones may have on patterns of residence. One factor is that residential neighborhoods rarely have precisely defined boundaries; schools provide local administrative boundaries which are widely known. A second factor

is that young couples with young children account for much of the extraordinary residential mobility of Americans. Choice of location depends on available housing, but also on perceptions of neighborhood amenities. Among these, the perception of the local school looms large. If it is identifiable as a black school, whites tend to move away or stay away. If schools were not racially identifiable, or if predominantly black schools were not perceived as inferior schools, then school attendance zones would play only a minor role in residential choices and in the behavior of the real estate business.

A third factor is that schools, their staffs and attendance zones, are subject to direct administration control. Assignment of a black principal and the shifting of a school attendance boundary are highly visible deliberate acts that may imply racial consequences to homeseekers, landlords with vacancies, and banks with funds to loan.

Finally, I call attention to the more general impact that school policy has on racial residential segregation. In Detroit, as in many other cities, the black population prior to the First World War was small (6,000 in 1910), and although blacks were residentially concentrated to a degree, there were no solidly black neighborhoods and no totally black schools. The patterns of nearly complete residential segregation and nearly complete school segregation grew up together. Either pattern, without the other, would have been much more difficult to foster and sustain. Although many forces other than school policies have contributed to racial residential segregation, I anticipate that desegregation of schools would have a cumulatively increasing effect on residential desegregation.

What Taeuber has just presented as hypothesis has, however, been taken as fact in the courts. For example, Judge Garrity wrote in his opinion in the Boston case (1974, p. 470):

> [20,21] There are various answers to the defendant's arguments: first, it is now generally recognized that school officials' practices may have a substantial impact upon housing patterns; second, when school officials have followed for at least a decade a persistent course of conduct which intentionally incorporated residential segregation into the system's schools, that conduct is unconstitutional; and, third, when school districting and a neighborhood school system are fraught with segregative exceptions, neither defense need even be considered.
>
> It is now generally agreed that schools and neighborhoods have a reciprocal effect upon one another. There is a substantial interaction: a school will cause the racial

composition of the neighborhood to shift and vice versa. Both plantiffs' and defendants' expert witnesses testified to this.

Summary

Perhaps I overstate the case, but to my view, Professor Taeuber's "perspective" is Judge Garrity's "fact", attested to, according to Judge Garrity, by expert witnesses for both sides. It would be interesting to see this evidence. In Judge Garrity's view, school administrators have used their discretion to help cause residential segregation. I do not think I overstate his argument to say that the logical implication is that if school administrators act differently, racial residential segregation may be lessened or reversed. Further, I think it is a reasonable extension of his thought to conclude he believes such a reversal would have an important effect on racial residential segregation throughout the entire city of Boston, and not just in a few isolated "neighborhoods". With this in mind, I would like to turn to some discussion of Boston itself.

7. SCHOOL AND RESIDENTIAL SEGREGATION IN BOSTON, AND SOME SPECULATION

I hope to put this general information into some focus by discussing the social demography of one city, Boston, before moving back to more general concerns. The city of Boston's school system is one which everyone, I think, would agree, has had large scale desegregation and large scale loss of white students from its public schools since 1974. After we consider these gross facts, we can turn to speculate on questions of its effect on residential segregation: in the city and suburbs, and even more speculatively, possible parallels for other metopolises. I think Boston is a good example of a city where knowledge of its segregation patterns would have given an indication of where violent conflict was likely, and its lessons are not irrelevant to similar heavily ethnic cities.

We can see how drastic has been the white flight from Boston schools from the most current enrollments. Professor Christine Rossell has kindly sent me statistics (I do not suggest she has changed her general conclusions or that she has tempered her criticism of Coleman's work, only that she is continuing her own research) which to my mind show white enrollment dropping precipitously since the official desegregation plan started. If we consider whites enrolled in Boston public schools in grades kindergarten through 12th grade, the percentage declines from the previous year in 1972, 1973, 1974 and 1975 are -3.3, -6.6, -14.9, and -19.9. These statistics, to my mind, are convincing evidence that the school desegregation disputes since 1974 have caused "white flight"; white enrollment has dropped by approximately one-third in the two years encompassed by the formal court-ordered desegregation plan.

I do not know if this could have been avoided. But regardless of what people might have done, the end result in that city's school system has been an enormous loss of white public school students. Perhaps another plan or other factors might have prevented white flight, but we shall deal with the actual situation.

Less noticeable, but just as striking to me are some other statistics which show that "minority enrollment", which includes Blacks plus Orientals, after having increased every year for a decade, fell -0.5 percent between 1975 and 1976. Moreover, the loss of Black students was even greater, -1.0 percent, a loss partially compensated by increased enrollments among "other minorities." I think this indicates Black students are leaving the system, and presages further declines. From 1968 to 1972, Black enrollments increased by one to two thousand each year. But in the years 1972, 1973, 1974 and 1975, Black enrollments were 31,728, 31,963, 32,265, 31,940. Thus, the year before the desegregation plan went into effect, Black enrollment grew only 235, then after the plan went into effect, Black enrollment grew 302, and then decreased by 325. I do not think this change in Black enrollment is entirely due to the turmoil of desegregation, but my guess is this turmoil is having some effect. As I shall speculate in more detail later, I think the drop in Black enrollment is in part the beginning of the Black middle classes moving away from their own poor, as with the whites of Boston; in the past this movement has taken place largely within the city, now it may go more outside. In general, both Black and white middle classes move to segregate themselves from the poor, and the current turmoil in Boston hastens the process.

This turmoil also may have an effect on the statistics gathered, which I suspect is related to the closing remarks in Appendix A of the Federal District Court examining the "accuracy of past enrollment data." I shall restrict myself here to just "white" enrollments. Rossell's data for the 1975-1976 school year come not from enrollment data as of the autumn months around October (as I assume the previous years do), but from figures as of February 6, 1976. I have obtained the Boston school district official reports for this latest year, and find in the report issued February 2, 1976 (used by Rossell) "Actual Enrollments" as of January 30, 1976, of 36,563. However, the "Actual Enrollments" in the report issued 3 months earlier on October 20, 1975 shows 35,687 as of October 8 and 17, for kindergarten through grade 12. This October to January difference is not a large one. But what makes it interesting is another category in these school documents, called the "Did Not Reports." For whites, as of October 17, 1975, these were 4,998; as of January 30, 1976, these were 1,191. I am no expert on Boston school statistics, but I would not expect to have such a category and certainly not one so large in numbers, in a smoothly running school system. I suspect it means children from last year's enrollment who are not accounted for by legally dropping out, or by known transfers to other systems. I do not wish to exaggerate what may be simply my own ignorance of Boston school statistics, but it is possible that the turmoil of the past two years may be the reason the Courts have questioned the accuracy of Boston school statistics.

We may now turn to the issue of this "white flight" on residential segregation. Basically I have no evidence concerning residential moves. Some losses undoubtedly reflect migration from the city, but some also reflect children, illegally enrolled in suburban public schools, or in private schools, with no residential moves by the family. My own guess is that much of this, for the moment, is change from public to private school enrollment, with little migration. Some migration will follow, particularly by middle classes; whether the child lives at home or with a relative in the suburbs, the family will follow. Obviously, this will vary by such factors as family income, or whether the children are in secondary or primary grades. However, I do not think any of these moves will drastically effect the levels of residential segregation, except to marginally increase the racial.

I think it would be irresponsible of me to create a specific numerical estimate of "white flight", something which should be a research project in itself. However, I think I can point out that this phenomenon in Boston points out the limitations of comparative studies; that is, both Coleman and his critics make the point that school desegregation only accelerates the inevitable demographic trend toward a highly Black school system or city.

This may be a reasonable general conclusion, but I doubt that Boston fits the mold. I think this because I expect when the 1980 census unfolds, we will see that the great wave of Black migration to northern cities ended during the 1960-70 decade, and 1970-80 reflects a greater importance of more local phenomena. That is, metropolitan suburbs are far larger than their central cities, so their own natural increase and inter-suburban migration are major factors in change while the central cities become demographic backwaters, dependent more on the steady changes of natural increase or decrease now that their explosive white flight and Black in-migration are diminished.

Although I do not know anything about such important conditions as the maintenance of housing stock, I suspect Boston City would have had a much higher proportion of whites than many of Coleman's 22 largest districts. Certainly, all the evidence points to a strong territorial attachment upon the part of the white ethnics. More important, I am struck by the 1970 census fact that Boston had relatively few caucasian newcomers such as Puerto Ricans, and its Black percentage had reached only 16.3 percent, at a time when so many other cities' Black percentage had soared to figures such as Chicago's 32.7 percent, Cleveland's 38.3 percent, or Detroit's 43.7 percent.

Thus, assuming the pressures of mass migration (of whites out, or Blacks in) is over, even the slow erosion of differential natural increase by race and ethnicity would not have transformed Boston into an overwhelmingly Black city such as Detroit was bound to be. I expect the levels of racial and ethnic residential segregation to remain the same because they have always been high, while levels of school segregation to drop under the present court-ordered desegregation program. But the percentage of whites in the public schools and the city will drop far below anything one might have projected prior to the turmoil of Boston's busing conflict.

Obviously if one defines more segregation as a city more heavily Black and suburbs more white, one has more segregation. I choose to use the Index Number approach because I see no evidence there was much integration inside or outside of the city. My ideas are based partly on my understanding of the literature which indicates that school changes have only a marginal effect on residential patterns, and also upon my understanding of segregation as it has existed in Boston. Although my evidence for social class segregation is impressionistic, derived from cartographic inspection, or based upon inspection of broad census categories, we do have sufficient knowledge of racial and ethnic segregation (Kantrowitz, 1975; Guest and Weed, 1976) in Boston to conclude it has been a highly segregated city and metropolis, with each factor existing in its own right, and not as a creature of others.

We can best begin with the central city in which the demographic evidence, though circumstantial, indicates that the level of racial segregation at least in elementary school has been overwhelmingly a product of residential segregation. I do not mean by this to claim the Boston School Committee has been free of (or guilty of) taint of gerrymandering; to my mind, whatever their attitudes and behavior in this respect may or may not have been, they are irrelevent in the fact of the high level of residential segregation. Whether one chooses to believe they were total innocents striving only to preserve neighborhood elementary schools, or vicious bigots determined to keep little children of both races apart, the end result of residential segregation would have been segregated elementary schools.

We know from Farley's (1975) comparative study that this school segregation in 1970 (measured by the Index of Dissimilarity) was 74. Social class or European ethnic statistics are not collected, so there is no evidence concerning sub-populations among white children. What level of segregation should one expect if the schools were ideal "neighborhood" schools? By calculating the Index between Negro and Non-Negro children aged 5-14 from 1970 census tract statistics, we arrive at an index of 89, even higher than the 81 (per Van Valley, 1975) level between all Blacks and whites of all ages. This can only be approximate comparison, but this difference of 89 residential, versus 74 school, is so large as to be persuasive. A conclusive accounting would require a large scale investigation into public versus private schools, as well as the history of Boston education. But the fact that residential dominates school segregation is emphatic.[2]

Although our census data are limited to race, place of birth of immigrants and their children, and such indicators of social class as family income, we can say Boston is strikingly segregated by income, ethnicity and race. Racial segregation is perhaps most pervasive and extreme (indexes of dissimilarity of 81.2 in the city, 54.4 in the SMSA ring, per Van Valey, et al., 1975: Table 1), and undoubtedly much of this is based upon prejudice and discrimination against Blacks.

Also, most reports (e.g., U.S. Commission on Civil Rights, 1975) indicate the wealthier have tended to move to the suburbs. Although I have no direct evidence, the census statistics suggest this tendency exists for both races, and if I am correct, it means the city and the public schools of Boston city may lose the middle class of both races. Data which illustrates my guess are in Table 1, a percentage distribution of white and black 1969 family income as shown in the 1970 census.

Table 1 shows the striking economic disparity between the Boston SMSA's white and Black families. For example, if we define "middle-class" as a 1969 family income of $10,000 or more, 28.5 percent of all Negro families and 61.5 percent of all white families would be so included. Also, Black families are overwhelmingly (83.2 percent) concentrated in Boston city, while few white families are (18.3 percent) so. But we can also see from this table that for each race, the higher the income, the greater the tendency to live outside Boston City itself.

More to the point perhaps are the census tract statistics for 1970. Here we see several enclaves of Black middle class families where the 1969 median family incomes exceed $10,000 (Newton, Medford census tract 3393, and Cambridge census tract 3546). Most interesting is the city itself, where the census tracts portray a crude gradient of the poorest Black families toward the center, the more affluent to the south. A southern census tract (number 1010) contains the only city concentration of Blacks with median 1969 family incomes over $10,000. This tract is just outside Boston's "B-Burg" line, an area within which local financial institutions made available special mortgage money for Black families. This special financing generated much controversy in Boston, but the greatest concentration (although extremely small -- 122 Negro families in a tract containing a total of 3,132 families) of wealthier Black families obtained housing out-

side the controversial area. In effect many wealthier Blacks avoided the special financing and the conflict. None of this should be taken to represent a claim that the racial millenium is upon us; blacks are still poorer than whites, and the acquisition of money by black families does not free them from white prejudice and discrimination. But it does indicate how Black families with more money live in different places than those with less. Moreover, my interpretation of the literature leads me to conclude that the Black middle class is no more likely than its white counterparts to either live among, or school their children among the poor of either race, especially if school integration does not lead them to believe their children will be better off for it. Consequently, my guess is that the current school busing dispute in Boston City will lead to a reduction of Black middle class children in the public schools, and probably in the city, and will tend to increase the residential segregation of middle-class Blacks from their own poor. Nor do I see this school turmoil giving middle-class Blacks any more reason to desire white neighbors. I guess it will marginally increase their desire for self-segregation from whites.

Our understanding of ethnicity in Boston is a function of assumptions concerning the evidence. For the most part, studies of ethnic segregation in American cities have relied upon decennial census statistics which encompass only immigrants and their children. Thus, for example, if we ask if Irish and Italian (who I assume are Roman Catholic in Boston) maintain a separatism from each other or others, we have statistics only for the "foreign stock", the first two generations. But in 1970, Boston's entire foreign stock encompassed only 37.0 percent of the city's 641,071 total population (plus 16.3 percent Negro, 1.8 percent Other Nonwhite, and 1.1 percent Puerto Rican by birth or parentage). Thus, 43.7 percent of Boston City's 1970 population is tabulated by the census as non-Puerto Rican whites who are native born of native parents or, NBNP. If one assumes, as do many scholars (e.g., Lieberson, 1963; Taeuber and Taeuber, 1964) that this NBNP category is sufficiently homogeneous to treat it as a single group, then one can compare, say, Boston City's 1970 Irish Foreign Stock against this NBNP category, and I am sure one will find that the segregation of the Boston Irish from NBNP has declined considerably over the last decades. It is this assumption which has underlay the work of numbers of students of ethnic segregation, and which has led them to conclude that there has been a considerable decline in the segregation of European ethnics, concomitant with a constant high racial segregation of Blacks from whites. This conclusion in turn naturally leads to the conclusion that racial segregation is unique, for it has remained high while the ethnic has declined. We alluded to this earlier when we outlined Hawley and Rock's conclusion that inter-ethnic segregation was a thing of the past. Parenthetically, we might add that if one appends to this an assumption that attitude surveys which show more acceptance of integration by an undifferentiated white (and Black) population are valid reflections of behavior, then it follows that the main props of segregation are racial bigotry, social class differences, and the manipulations of such institutions as banks. Thus, the existence of ethnic

segregation is the hinge upon which the whole argument turns. It is important to realize that the only empirical evidence that ethnic ssegregation is past are a few studies using census statistics which assume the NBNP category is in some way homogeneously "non-ethnic".

To my mind, racial bigotry, social class differences, and institutional manipulation are important. But so also is voluntary ethnic separatism. How important will be discussed next, but I think it necessary to point out that I make no universal claim for the importance of European ethnicity in segregation in American cities. It would be delusion to believe that a nation of more than 200 million people spanning a sub-continent could be so characterized. However, I think it relevant that a number of school districts among Coleman's prime 22 candidates for white flight are similar to Boston in this respect, such as New York, Cleveland and Chicago.

Fortunately, we do have some collateral evidence for Boston City, the "Police List", a roster of every adult of voting age by address, the basic roster used to check voter eligibility. This list is in constant use by numerous agencies, including the election board, and is generally accepted as reasonably complete; however, there has never been a detailed study of its completeness as an adult census.

Evidence derived from the Police List leads me to conclude that the 37.0 percent U.S. Census enumerated "Foreign Stock" stands surrogate to at least 50 percent, and probably two-thirds of the city's population. If we add to this the Negro, Oriental and Puerto Rican populations we then would have 70 to 85 percent of the city's population. If so, then at least for Boston, we shall see that Hawley and Rock's conclusions do not hold.

In January 1975, under the direction of Dr. Rolph Goetze and under the supervision of Mr. George Curuby (for the Boston Home and School Association), selected areas were picked as representative of heavily "Italian" and "Irish" neighborhoods in Boston City. Two women, lifelong residents, went through each name in the 1970 Police List for these areas. One woman, ethnically Irish, reviewed the Irish neighborhoods and categorized each surname as "Irish or not; the other, ethnically Italian, did the same for Italian neighborhoods.

Table 2 presents results we might expect were there a heavy ethnic concentration of one group. We see that for the Irish neighborhoods, at least 60 percent of the residents had Irish surnames, and were presumably "Irish"; similarly, we judge that in Italian neighborhoods, well over 80 percent were "Italian". Granted these percentages are but approximations (for example, an ethnically Irish wife could have an "Italian" name), but the magnitude of these percentages confirms the commonsense knowledge of the people of Boston that many neighborhoods are heavily ethnic. Moreover, this does not exhaust ethnicity; for example, the approximately 40 percent of the population in precincts 3 and 6 in Dorchester who were not "Irish" are not necessarily "non-ethnics"; indeed, we expect these represented concentrations of other identifiable groups.

By map inspection, we have identified in the last column of Table 2 the census tracts which overlapped these voting precincts. In Table 3, we have listed these tracts with their percentages Irish and Italian Foreign Stock, as well as percent Negro. In the first two blocks of tracts, we have placed tracts of "Irish" neighborhoods; the census enumeration indicates small concentrations, approximately 15 percent of the population. The police lists and the census tracts suffer problems of comparability such as the difference between an adult (Police Lists) and a total (Census) population, or between surnames and place of birth. But irrespective of details it seems reasonable to conclude that where one finds Boston City census tracts with few Negroes or few persons of any Foreign Stock enumerated along with about 15 percent or more Irish Foreign Stock, one may safely guess that at least 50 percent of that tract's population are probably "Irish". Similarly with the blocks of "Italian" census tracts, we think it reasonable to think that tracts with an enumerated majority of Italian Foreign Stock, but no appreciable Negro or other Foreign Stock, one may safely guess that at least 75 percent of that tract's population are "Italian". Any extrapolation one wishes to make from this evidence is necessarily subjective, but we think even the most conservative judge would allow that the overall 37.0 percent Foreign Stock in the city's 1970 population is representative of at least 50 percent of the total population;[3] in our own judgement, we think it represents over 60 percent. Whatever percentage one takes, however, we believe we have established that a majority of the city's population can be considered European ethnics, and together with the Black, Latin, and Oriental populations, encompass 70 to 85 percent of the people of Boston.

With this background, we may review a few figures from a longer study of Boston segregation (Kantrowitz, 1975). The years 1930, 1950, 1960, and 1970 are particularly apropos because the Black population grew from 2.6 to 5.0 to 9.1 and finally to 16.3 percent of the city total. Thus, Blacks grew from a minute percentage to a substantial minority. For these years, the average segregation levels among just European ethnics (for example, Irish vs. Italian, etc.) were 53, 51, 51, and 55. In effect, inter-ethnic segregation remained the same, irrespective of percentage Negro, and to my mind could not have been a response to the Negro population increase. This is not to say that the Black population was not discriminated against, for during these years the average segregation of Negroes to European ethnics averaged 81, 82, 80, and 85. I think the difference between the average 50 in inter-ethnic, and the average 80 in ethnic-Negro reflects prejudice and discrimination against the Boston Black population. But this discrimination is neither new nor unique; rather is an intensification of a long standing phenomenon. In effect, one may attribute to the white ethnics any social psychological attitudes one wishes, from angels to bigots; but they have maintained a stable spatial separatism over at least 40 years which seems to me independent of their own relative decline, or the Black increase. I don't think one has to believe the white ethnics are pure of either motive or neighborhood to conclude they have maintained a kind of territorial imperative far beyond reasons of racial bigotry.

Moreover, 1970 segregation is strong among populations where race could not possibly be a factor. For examples, that between Irish and Italian foreign stock is 51; the segregation of Irish and Italians from newcomers such as Mexicans, Cubans, and Puerto Ricans is about equal to their segregation from Blacks. Finally, one may turn to segregation among what are most likely Roman Catholic Latins themselves; the segregation of Mexicans from Cubans is 80, from Puerto Ricans is 86, and that of Cubans from Puerto Ricans is 79.

If we move to the suburban ring itself, we no longer have the evidence of the Police Lists, so we cannot be sure the census data mean the same thing. But generally, the suburban foreign stock shows no average decline in inter-ethnic segregation with the notable exception that the two largest ethnic populations, the Irish and Italians drop from 51 in the city of Boston, to 32 in the suburbs. Also, the segregation of Blacks from European ethnics drops from 86 in the city to 75 in the suburbs. (Van Valey, et al., shows an even sharper drop in the racial segregation of all Negroes from all whites of 81.2 in the central city to 54.4 in the suburbs).

Thus, I see no reason to believe that segregation patterns are generally diminished when we move from the city to the suburbs. True, a few have decreased, but most have not. One striking decrease however is the racial; social class may be part of the answer, but I suspect that it reflects also suburban land development plus greater racial tolerance. However, I don't think this is any prelude to large scale integration, and in fact the consequences of the City's school conflict may tend to work against further integration.

There remains another question: what might happen if school desegregation became metropolitan instead of limited to just the city. This would change several smaller districts into a single large one, with the problems of greater size. In such a situation, we may hypothosize any of a number of alternatives. However, let us consider the most extreme, a mandatory busing program which encompasses all of central city and suburb. My own guess is that there would be little residential movement or segregation increase because the metropolis is so large (the Boston SMSA is 1,233 square miles); there would be no place to go except for those already at the edges. Under such circumstances I doubt that patterns of segregation would change much, except for a small increase in racial segregation. Taking into account the cost and logistics of such a program, the metropolis might be divided into segments; in such a case the "brownian movement" of population change would create shifts in residence and increase racial segregation; but again, I think segregation is so tenacious and conservative a force that there will be no massive change. To my view, if a bitterly opposed school integration plan were put into effect, it would more likely destroy public schools than it would destroy residential segregation. I have presented one extreme. At the other, a broadly acceptable plan might produce not only stability, but the kind of integration hypothesized by Taeuber. Since I know nothing of either law or school organization, perhaps such an acceptable plan is possible for the entire metropolis.

In the end, I do not think Boston is entirely unique, and looking at Coleman's largest 22 school districts, I judge that several such as New York, Cleveland, and Chicago have strong enough enough ethnic levels of residential segregation that their experiences would be similar to Boston. Again, as Coleman and his critics agree, the smaller districts would not be effected, in general. I suspect that if smaller city districts were unified to absorb the suburbs, some of these would have sufficient strains as to increase racial segregation - but again only slightly. Of course, some of these unified smaller districts could experience turmoil as has Louisville.

Summary

Boston is one of Coleman's largest 22 districts which has seen "white flight" from its public schools since its desegregation program. It is entirely possible that a different kind of plan, or a different response on the part such people as community leaders might have prevented that city's turmoil. Unfortunately, I have no basis for evaluating these hypothetical possibilities. Moreover, there is no basis for claming this "proves" Coleman right because logically it could be just as much one of the expected "exceptions" to the findings of Coleman's critics. Finally, Boston is not "typical" of Coleman's largest 22 districts, much less cities in general. But neither is it so "untypical" of other cities such as New York, Cleveland, or Chicago with regard to such factors emphasized by Coleman as a relatively large Black inner city population and small suburban one, coupled with a characteristic I consider critical, a relatively high and stable voluntary inter-ethnic segregation.

Because of this strong pattern of residential segregation, while I expect the "white flight" to cause substantial loss of whites from the city, I doubt that it will create drastic changes in the level of segregation in either city or suburb. Moreover, if a school desegregation plan were to include the entire metropolis, city and suburb, I expect it would reduce the demographic consequences even further; there would be less migration because there would be no place to move to except for those already living at the edge, and segregation patterns would remain the same. Of course, if the plan were bitterly opposed throughout the entire metropolis, there might be loss of whites (and perhaps Blacks) from the public schools to private ones. As long as we are speculating, it is also necessary to add that an "acceptable" metropolitan plan might also be possible.

CONCLUSION

If we return to our original question, what are the effects of public school desegregation on residential segregation, most research indicates that in general, there is very little. Only Coleman's analysis of the largest 22 districts projects white flight. This is limited demographic consequence whether we speak simply of "white flight" from the schools and city, or whether we speak in terms of segregation index numbers. Moreover, even where school desegregation does have demographic consequences, all concerned view these as one-time effects, which serve only to accelerate the existing trends.

Given such consensus, one might expect a research millenium had arrived, with so many diverse efforts leading to similar conclusions. The major divergence has been the findings of James Coleman and his associates, that among the largest 22 school districts, large scale school desegregation would lead to "white flight"; to my way of thinking, after we eliminate districts where the returns are already in (e.g., Atlanta, Boston, Tampa, etc.) we are left with about a dozen school districts where the issues are still in balance. So far as research is concerned, Coleman's conclusions seem a reasonable possibility, and so far as social policy is concerned, it would be a large but manageable job to assess the chances for each of these dozen. Just as there will be exceptions to the rule that smaller school districts probably will not have "white flight", so, if Coleman is right, would there be exceptions among the largest districts. Rather than an intensive effort to see where the disagreement lay, the debate has exploded into a polemic far beyond the confines of this paper.

Perhaps this polemic will contribute to a lessened use of social research in public policy. To my understanding, the NAACP Legal Defense Fund, Inc. already views social science as irrelevant to decisions concerning school desegregation. After reading Wolf (1976) on Detroit, and the Federal Court of Appeals on Boston (Appendix A), I suspect perhaps the Federal Judiciary will soon share their opinion. It may be that the ideological gap is too great to use research, even if there is agreement on fact. For example, all sides agree that where there is "white flight" attendant upon school integration, it is but a one-time effect intensifying the existing trends. Coleman's point is that this is bad because it hastens irreversibly what it was designed to prevent, while his critics contend it is further proof that school desegregation has no effect on demographics.

In addition, there are hypotheses, such as those by Taeuber, which envision school desegregation plans which might bring whites back into the city schools, or additional plans which would involve metropolitanization of the school system. Here we move into hypothetical areas outside of much empirical knowledge, except we can say a plan which merges city and suburb by definition excludes "white flight" by residence, except for those already at the edge of the metropolis. In such a case, the salient issue would be the relationship of public and private schools, something outside our demographic concerns. The only population movement would be of concern would be if the cost and logistic problems of metropolitanization led to segmentation of the metropolis--then, we would be back into a modified version of the questions of central city versus the suburbs.

Footnotes

[1]I have never done research in schools, education, or their desegregation, so my views on these and their legal complications are those of a layman. My only contact with work in this field has been as a consultant on residential segregation (and its probable implications for school segregation) to the Boston Home and School Association (for whom I carried out a study), and brief discussions with the staffs of the County Judge of Jefferson County (Louisville), Kentucky, and the Cleveland Board of Education.

[2]From the published census tract report (U.S. Census Bureau, 1972), Table P-5, Negro Children aged 5-14 years, in Census Tracts with 400 or more Negro population was used in conjunction with Table P-1, all children aged 5-14. The 24,429 Negro children aged 5-14 in Table P-5 constituted 93.4 percent of the 26,147 Negro children aged 5-14 enumerated by the 1970 Census in Boston City (U.S. Census Bureau, 1973a:66).

[3]Consider just the 24 census tracts with at least 15.0 percent Irish foreign stock. We discard one tract (number 1501) with 20.6 percent Negro population. The remaining 23 tracts have few Negroes, and for the most part the remaining population is native-born-to-native parentage (NBNP), that is, without any census ethnic identification. For the 107,850 total population of these 23 tracts, 18,788 (or 17.4 percent) are Irish foreign stock, 894 (or 0.8 percent) are Negro, and 60,690 (or 56.3 percent) have no census ethnic identification, for they are NBNP. If we assume that fifty percent of the total population of these tracts is "Irish", we encompass approximately 35,000 "Irish" third and subsequent generation from the 60,690 NBNP. These 35,000 alone equal approximately 5 percent of the total population of Boston City. We think it reasonable to believe that this estimate plus others possible of the "Irish" in remaining tracts, as well as Italians and other ethnics, would easily raise the existing 37.0 percent foreign stock to over 50 percent ethnic.

Table 1

Percentage of Boston SMSA Families Who Live Inside Boston City, For Each Income Bracket of 1969 Family Income, Negro and Non-Spanish Language Whites

Income	Negro		White	
	Number in SMSA	% Who live in Boston City	Number in SMSA[1]	% Who live in Boston City
All Families	28,160	83.2	625,540	18.3
Less than $1,000	1,442	89.0	8,592	28.3
$1,000 to $1,999	1,233	91.6	9,707	26.7
2,000 to 2,999	2,320	90.6	15,746	28.5
3,000 to 3,999	2,535	90.4	17,929	29.2
4,000 to 4,999	2,432	89.0	20,673	27.3
5,000 to 5,999	2,391	85.0	23,801	26.3
6,000 to 6,999	2,329	88.9	27,891	25.3
7,000 to 7,999	1,889	80.8	34,456	23.8
8,000 to 8,999	1,826	85.3	40,285	22.1
9,000 to 9,999	1,746	79.9	41,487	20.5
10,000 to 11,999	2,904	80.1	86,302	17.5
12,000 to 14,999	2,692	75.4	103,306	15.9
15,000 to 24,999	2,167	64.0	145,169	13.3
25,000 to 49,999	223	60.1	41,786	9.6
50,000 and Over	31	45.2	8,410	6.4

Source: U.S. Bureau of the Census, 1973, Vol. 1, Part 23, Tables 89, 94, 100.

Table 2

Irish and Italian Surnames As a Percentage of All Surnames, Persons Aged 20 and Older, Selected Voting Precincts, Boston City, 1970

Neighborhood, Ward, and Precinct	Total Adult Population	Number	Percent of Total	Identification of Census Tracts which overlap some part of given Precincts
		Irish Surnames		
Uphams Corner				
Ward 13, Precinct 6	1,539	939	61.0	912,915
Ward 13, Precinct 9	1,546	1,012	65.5	911,912,915
Dorchester				
Ward 16, Precinct 3	1,547	948	61.3	922,1005
Ward 16, Precinct 6	2,212	1,338	60.5	1006,1008
		Italian Surnames		
East Boston				
Ward 1, Precinct 2	1,527	1,303	85.3	504,505,512
Ward 1, Precinct 3	1,447	1,035	71.5	503,504,506,512
North End				
Ward 3, Precinct 2	1,762	1,447	82.1	301,305
Ward 3, Precinct 3	1,736	1,497	86.2	301,302,304
Ward 3, Precinct 4	1,687	1,573	93.2	203,301,302

Source: Police Census, Boston City, 1970

Table 3

Percent Irish and Italian Foreign Stock, and Negro Population, Selected Census Tracts, Boston City, 1970

Census Tracts	Total Population All Ages	Percentage of Total Population which is: Irish Foreign Stock	Italian Foreign Stock	Negro	Map Inspection Comment vis-a-vis Neighborhoods in Table 5
911	6,281	18.1	2.5	0.2	Partial overlaps
912	3,571	9.7	3.1	1.5	only, Uphams
915	5,660	10.6	3.4	6.6	Corner
922	4,315	19.3	3.8	1.3	Partial overlaps
1005	7,396	9.8	2.5	12.5	only, Dorchester
1006	9,637	20.5	4.0	0.5	
1008	6,114	19.7	4.2	0.2	
503	1,793	3.6	18.7	8.9	Though not co-
504	2,096	0.3	42.9	0.2	terminus, tracts
505	1,782	0.3	67.7	0.2	504 and 505 seem
506	1,498	0.5	48.2	0.1	almost entirely
512	3,074	1.5	32.1	0.2	within East Boston
203	4,285	4.1	5.9	4.1	Though not co-
301	3,204	0.0	57.4	0.1	terminus, tracts
302	1,911	0.8	69.0	0.0	301,302,304, and
304	3,033	1.1	63.4	0.8	305 seem almost
305	1,916	1.2	64.1	0.2	entirely within the North End

Source: U.S. Bureau of the Census, 1972

Appendix A

Morgan v.s. Kerrigan, U.S. Court of Appeals For the First Circuit
January 14, 1976, p.30-31.

Subsequent to the district court decision, we allowed to be filed, subject to a determination of relevance, voluminous affidavits and other materials by social scientists on the subject of white flight.

The admissibility of these submissions have been attacked as being outside the record, F.R.A.P. 10 (a), and as hearsay. The materials have been defended as sociological data in the nature of "legislative facts" relevant to a determination of the law governing the district court in this matter.

For reasons we discuss in the text, we reject all these materials as irrelevant to the issues before us on this appeal. We include a brief synopsis of these materials to illustrate the difficulty in evaluating white flight if it were relevant.

The data submitted initially by the Association consists of an affidavit and report prepared by James S. Coleman, Professor of Sociology at the University of Chicago. Dr. Coleman states that his recent study shows that while there is decreasing segregation within school districts, segregation between school districts in the same metropolitan area is increasing; that rapid increase in loss of white children from central city schools follows immediately after school desegregation; and that as a consequence, desegregation has not significantly raised the levels of academic achievement of blacks.

Plaintiffs counter with the transcript of testimony given by Jane R. Mercer, Associate Professor of Sociology, University of California at Riverside, in a case involving Indianapolis. Her testimony shows that she studied desegregation in school districts throughout the state of California; that white exit to private schools is a short term phenomenon; and that declines in white population in the cities are part of a long term demographic trend independent of desegregation. The plaintiffs also submit a paper written by Meyer Weinberg, Editor of Integrated Education magazine, which summarizes other studies showing white flight to be an avoidable phenomenon, not an inevitable consequence of mandatory desegregation. Weinberg's paper and another paper written by Professor Green of Michigan State University and Pettigrew of Harvard University, criticize Dr. Coleman's methodology, claiming that the source of his raw data is unknown; that failure to evaluate large cities which have been subject to massive desegregation orders separately from large cities which have not been subject to court orders undermines the study's relevancy; and that failure to control for other variables, which may be correlated with white flight, jeopardizes the validity of his conclusions.

Dr. Coleman replies in an affidavit filed with a copy of a working paper, "Trends in School Segregation, 1968-73". In his affidavit, Dr. Coleman states that the report previously filed with the court was prepared for oral delivery and was based on the attached working paper. He defends his methodology, states that his conclusions are consistent with the findings of the studies cited by Weinberg, and the studies conducted by Dr. Mercer. Dr. Coleman states that his

study shows that massive white flight will occur when there is a significant decrease in segregation in a city where there is a high proportion of blacks in the central city and suburbs of a significantly different racial composition.

Plaintiffs, in rebuttal, file another study prepared by Christine H. Rossell, of Boston University. The Rossell study, prepared from data on 86 northern school districts subject to court ordered or legislatively enacted school desegregation, suggests that white flight is minimal and a temporary reaction to school desegregation. Plaintiffs claim that Dr. Rossell's study differs from Dr. Coleman's in that Dr. Rossell deals only with northern school districts subject to desegregation plans while Dr. Coleman does not distinguish between forms of desegregation.

In the final submission, Dr. Coleman defends his study against the Rossell findings, suggesting that her analysis is inadequate to examine the effects of desegregation on a core city school system. He further claims that his model has proved accurately predictive of the Boston experience.

Throughout this series of submissions this court has been burdened with reports written for sociologists by sociologists utilizing sophisticated statistical and mathematical techniques. We lack the expertise to evaluate these studies on their merits. We do come to one conclusion, however. The relationship between white flight and court ordered desegregation is a matter of heated debate among experts in sociology, and a firm professional consensus has not yet emerged.

Appellants have also filed with this court copies of the Boston School Department's current census of students according to race and minority group. The figures facially suggest loss of a significant number of white enrollees. We note, however, that the district court is currently studying the accuracy of past enrollment data. We decline to make any conclusions concerning the existence or nonexistence of white flight on the sparse figures available to us.

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Appendix B

James Coleman, "Recent Trends in School Integration", paper presented

Conclusions Drawn by Author

Pages 1-15

The following are abstracted according to my judgement.
p. 7-11 Segregation in 1968 (Analysis based on inspection of Tables)
Table 1, (by region): with some variation, "Probability of Contact" is low; total segregation is high, especially in the South; within district segregation varies considerably.
Table 2, (by size of district): "Probability of Contact" is variable: smaller districts show less segregation than larger.
Table 3 (within district segregation for regions by size of district): among large districts, segregation is high across all regions; among small districts segregation is low except in South.
p. 11-14 Segregation Trends 1968-72 (Analysis based on graphs)
Figure 1: total segregation decreased in South, increased slightly in Northeast and Midwest. Figure 2: within district segregation declined to some extent (everywhere except in middle Atlantic Region), and comparison with figure 1 suggests segregation between districts increased. Figure 3: (within district segregation by size of district): Segregation remained high in larger districts, but declined in smaller.
p. 14-15 Segregation, 1972, Table 5 is partially equivalent to table 3; among larger districts, segregation decreased only in South. (this conclusion goes beyond the data of these tables)

Pages 15-20 ("White Flight")

This section focuses on: "Decline in white population 1970-73 as a function of increase in proportion of black schoolmates of the average white in the two preceeding years 1968-70, and the proportion of blacks in 1970". (p. 17) 19 largest districts: "the impact of desegregation... on whites moving out of the central city is great" (p. 18).
1. If 0% blacks in 1970, 0 increase in blacks, 1% of whites leave during 1970-73. 2. If 50% blacks in 1970, 0 increase in blacks, an additional 9% of whites leave, 1970-73. 3. If an "average" % of blacks in 1970, 5% increase in blacks during 1968-70 an additional 10% of whites leave 1970-1973. Next 50 largest districts: desegregation has no additional impact on whites leaving.

at American Educational Research Association, April 2, 1975.

Population and Source of Data	Variables, and/or Indexes of Segregation	Methods of Analysis, Evidence Presented
Number of Black and white children enrolled in public elementary and secondary schools combined; for each school; presumably almost all U.S. school districts are included; 1968-1973 (alternate years of sampling); Classification by region, and by size of school district. Source: Office of Civil Rights, HEW. No evaluation of quality of the statistics.	Percent white; Percent Black; average of proportion in each school of white children weighted by number of black children (and vice versa) This "Probability" of Black child's contact with a white (and vice versa); when this "Probability" is divided by proportion of white children entire school system, it is called a measure of segregation, which in turn may be expressed as total, within-district, or between district.	Tables of variables by region, size of districts; 1968-72 changes in segregation index by region; graphs of 1968-72 changes in total and within-district segregation by region and size of district (graphs difficult to read.)
As above, but restricted as follows. The 20 largest central city school districts in U.S. (actually 19; Washington D.C. excluded because nearly all Black); also for the next largest 50 central city school districts. Unclear if data include just school enrollment or also census statistics. Selection based on 1972 enrollment. Note: These are largest 70 central city school districts, not cities.	As above. However, since "leaving the district" (p.15) is the dependent variable, we presume only school enrollment statistics are used, and not census statistics of residence. Text sounds as if residential migration statistics are used rather than inferred.	No data presented. Presumably regression equations used to project expected proportions of whites who would leave the school districts (dependent variable) under varying conditions.

SCHOOL SEGREGATION, RESIDENTIAL SEGREGATION: SOME SPECULATION

Nathan Kantrowitz, Kent State University

Residential segregation shapes school segregation. But what if we ask the reverse: what are the implications of school segregation for residential segregation? Answers range from "nothing" to "everything", depending upon one's readings of the urban scene, which in turn derive from assumptions and definitions concerning the nature of social life, and the meaning of empirical evidence.

The beliefs of those who answer "nothing" range: from everyone will accept it, to the children will transfer to private schools. The beliefs of those who answer "everything" also range: from people will flee, to people will be drawn to the affected areas. These in turn stem from differing beliefs over such phenomena as voluntary and involuntary segregation, or the importance of race, social class, or ethnicity in housing choice. Moreover, empirical evidence depends upon judgment; for example, demographers depend upon decennial census questions as well as choice from a variety of measures of segregation.

The author reviews the scholarship in these areas, particularly the research on the relationship between school desegregation and "white flight" through 1975. He concludes the various protagonists have used differing assumptions and definitions to talk past one another, so their differences have no bases in empirical fact. He suggests their contribution to social policy has been to obscure and confuse the courts.